LAMIAE PISCIS. CAPVT.

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Arthur Everett Shipley

Contents.

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THE HARVEIAN ORATION, DELIVERED BEFORE THE ROYAL COLLEGE OF PHYSICIANS, ON MONDAY, OCTOBER 19th, 1903.

W. H. ALLCHIN, M.D.

THE HARVEIAN ORATION

Structure and Function

THE

HARVEIAN ORATION

Delivered before the Royal College of Physicians
on Monday, October 19th, 1903

BY

W. H. ALLCHIN, M.D.

FELLOW AND CENSOR OF THE COLLEGE
SENIOR PHYSICIAN TO THE WESTMINSTER HOSPITAL

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TO

SIR WILLIAM SELBY CHURCH, BART., K.C.B., M.D.

PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS

OF LONDON

AND TO

THE FELLOWS OF THE COLLEGE

THIS ORATION DELIVERED BEFORE THEM

IS DEDICATED

THE

HARVEIAN ORATION

1903

Mr. President, Fellows of the College and Gentlemen,—In proceeding to discharge the duty which you, Sir, have laid upon me, and for which honour I desire to express my grateful appreciation, my course is in great measure laid down for me by the injunctions of the illustrious Fellow of this College whom we meet to-day especially to commemorate. In the deed by which he conveyed to the College in 1656, a year before his death, his estate at Burmarsh in Kent, Harvey provided that :—

> "There shall be once every year a general Feast for all the Fellows; and on the day when such Feast shall be kept some one person of the said College shall be from time to time appointed, who shall make

an Oration in Latin[1] publicly in the said College, wherein shall be a commemoration of all the benefactors of the said College by name, and what in particular they have done for the benefit of the said College with an exhortation to others to imitate those benefactors, and to contribute their endeavours for the advancement of the Society, according to the example of those benefactors; and with an exhortation to the Fellows and Members of the said College to search and study out the secrets of Nature by way of experiment, and also for the honour of the profession to continue in mutual love and affection among themselves, without which neither the dignity of the College can be preserved, nor yet particular men receive that benefit by their admission into this College which they might expect; ever remembering that *concordiâ res parvae crescunt, discordiâ magnae dilabuntur.*"

Herein may be read the whole duty of the Harveian orator, and I might occupy my hour and your attention, on this the 247th anniversary, by a recapitulation of those who have materially benefited the College by their gifts and endow-

[1] The Oration has been delivered in English since 1864.

ments, as well as of those who by precept and example have added to the lustre of our College and have benefited mankind by their pursuance of Harvey's exhortation "to search out the secrets of nature." Not a few of my predecessors in this honourable office have with much learning and in graceful diction fulfilled their task thus, as others have preferred to consider some aspect of Harvey's work in the light of more modern knowledge, or to show how fruitful have been the methods inculcated by Bacon, which Harvey was one of the first to apply, in extending the domain of science in those branches more closely associated with our own. Others again have pictured for us in eloquent phrase the times in which Harvey lived, and have called up to our appreciation his companions and contemporaries and the influence they exerted on the thought and progress of his age. And in this brief survey of how the theme has been treated it would ill become me to omit a reference to the masterly oration of our learned Harveian librarian who sought to show in true light the relation of Harvey to his great predecessor Galen, and how mistaken some of the notions concerning that relation have been. Further, too, he has pointed out that notwithstanding a large amount of very faulty anatomical knowledge that was consider-

ably corrected by the seventeenth century, "Galen's sources of evidence respecting the motion of the heart were the same as Harvey's—viz. comparison of structure in a variety of animals, argument from the use of these structures, observation of the living heart, and numerous experiments on animals."[1]

I am not concerned, even were it needful, again to tell the oft-told tale of Harvey's great discovery and how he demonstrated the truth thereof, for that it was that made his epoch-making work the glory that it is; nor yet to defend his claims to the full merits of all he did against the pretensions of those who sought to be-little it or even boldly aimed to usurp what to him alone belonged. This has been done more than once and done completely. Nothing I could say could add one iota to the justification; to attempt it were almost to cast a doubt upon its truth.

Briefly would I commemorate the good deeds and the munificence of our founders.

And first, King Henry VIII who by Charter in the tenth year of his reign (1518) incorporated this College, moved thereto in great measure by Cardinal Wolsey, but essentially at the instigation of one of his Physicians—Thomas Linacre—whom on that account

[1] Harvey and Galen. The Harveian Oration for 1896 by J. F. Payne, M.D.

we revere as our special Founder, and also our first President. It was he who provided the first home for the College in his own house in Knightrider Street, where the Fellows continued to meet for nearly a Century (1614) and who also laid the foundations of our Library.

To him succeeded 30 years later, in the Office of President, John Caius, whose name is perpetuated in that of a Cambridge College and who stands in especial honour in this our Fellowship as its munificent and enlightened benefactor. To him we are indebted for the institution of the College Annals, the earlier records of which he collected and wrote out in his own hand with an account in full detail of the College doings subsequent to his own election as a Fellow; and with scarce a break the proceedings of the College have been recorded by successive Registrars to our own days. From Caius also we received a revision of the Statutes, and the silver caduceus, the emblem of the President's office and carried by each one as such since Caius' time.

Also William Gilbert, our President in 1600, who "opened the modern era by treating Magnetism and Electricity on a scientific basis," [1] and whose fame has recently been commemorated by the formation of a Gilbert Society and the publication of a sumptuous translation of his great work, the work which induced Galileo to turn his mind to the subject. To the College Gilbert bequeathed "his whole Library, globes, instruments, and cabinet of minerals." [2]

Also Dr. Richard Caldwell, greatly distinguished, who

[1] Dr. Larmor, F.R.S. Introductory Address, Section A, British Association for the Advancement of Science, 1900.

[2] For much of the information contained in this section the orator is indebted to the Roll of the College of Physicians, by the late William Munk, M.D., F.R.C.P., Harveian Librarian.

together with Lord Lumley was the generous donor in 1582 of a rent charge upon certain lands, to endow a Surgery lecture in the College—the Lumleian Lectureship, the fourth holder of which was Harvey himself, who in his very first course (1615) set forth his views on the circulation of the blood.

Also Dr. Theodore Gulston, celebrated for his theological no less than for his medical learning, who bequeathed by will in 1632 £200 to the College "to purchase a rent charge for the maintenance of an annual Lecture to be read within the College sometime between Michaelmas and Easter by one of the four youngest Doctors in physic in the College."

Also Sir Theodore Mayerne (1573–1654), who left us his Library, including many manuscripts.

Also Sir W. Paddy, first time President in 1609 and again in 1618, who bequeathed £30 to the College in 1634.

Also Dr. Baldwin Hamey, Senior, who also left to the College a like sum after his death in 1640.

Also Dr. Baldwin Hamey, Junior, "the most munificent of all the benefactors of our College," as Dr. Munk describes him. In the troublous times of the Civil Wars, when the building rented by the College in Amen Corner from the Chapter of St. Paul's was like to be sold to pay the exactions levied in the City of London, Dr. Hamey himself "became the purchaser of the house and garden and afterwards gave it in perpetuity to his colleagues," and who besides contributing liberally to the Fund for rebuilding the College after the Fire of 1666 also "at his own sole cost amounting to some hundreds of pounds, wainscoted the caenaculum with fine Spanish oak with fluted pilasters ornamented with capitals, an elegantly carved cornice, and his coat of arms and crest immediately over the entrance." A portion of this wainscoting was removed

from the old College building in Warwick Lane and now adorns the Censors' room adjoining. Further, in imitation of Harvey he settled on the College the estate and manor of Ashlins in Essex, the proceeds from which were for the purpose among other things of doubling the premium to the Harveian orator and furnishing certain gratuities to the President, the remainder to be applied to the general purposes and advancement of the College. It is to Dr. Hamey's bequest that every Fellow present on the occasion of the election of our President owes the half crown in lieu of a pair of gloves which he then receives.

Also Dr. George Ent, who delivered the Anatomical Lecture in 1665, and who was knighted in the College by King Charles II., who had attended the discourse. Five years later he filled the Presidential chair. He was on terms of friendship with Harvey, and it was through him that the Master's work on the Generation of Animals was published; a benefaction to Science no less than to this College, which also received from him a pecuniary bequest.

Also Henry Marquis of Dorchester, who was admitted a Fellow of this College in 1658, having three years previously presented to the College £100 with which to augment the Library.

Also Dr. William Croone, who at his death in 1684 "left behind him a plan for two Lectureships which he had designed to found; one to be read before the College of Physicians with a sermon to be preached at the Church of St. Mary-le-Bow; the other to be delivered yearly before the Royal Society upon the nature and laws of muscular motion." His will, however, contained no provision for the endowment of these Lectures, and the funds for the two Lectureships were subsequently provided by his widow, who became Lady Sadleir.

Also Dr. Richard Hale, who left us at his death in 1728 the sum of £450, which with £50 given in his lifetime, was to be expended in the purchase of books.

Also Dr. Richard Mead (1673—1754), to whom we are indebted for this bust of Harvey.

Nor in this enumeration of our older benefactors is it fitting to omit him whom we to-day especially commemorate. Harvey at his own request added and furnished a Library and Museum to the building that the generosity of Dr. Hamey had provided for the College, most of which unfortunately was destroyed in the Great Fire. In July 1656, at his last attendance at the College within a year of his death, he "put the crowning act to his munificence by giving to the College in perpetuity his patrimonial estate at Burmarsh in Kent, then valued at £56 per annum." In his will also he thus further testifies his affection for the College. "Touching my books and household stuffs, pictures and apparel, of which I have not already disposed, I give to the College of Physicians all my books and papers, and my best Persian long carpet, and my blue imbroyedyed cushion, one pair of brass irons, with fire shovell and tongues of brasse, for the ornament of the meeting room I have erected for that purpose." He further directed Drs. Scarborough and Ent to select from his Library and collections such as "they shall think fit to present to the College, and the rest to be sold, and with the money buy better."

Passing to the benefactors of more recent date it is my pleasing duty to mention :—

Mrs. Bradshaw, who in 1875 bequeathed £1,000 consols to found a Lecture to be delivered annually in memory of her husband, Dr. William Wood Bradshaw, a Member of this College. A similar bequest was made to the Royal College of Surgeons.

Dr Gavin Milroy, a Fellow of this College, who in

1886 bequeathed a sum of £2,000 to establish a yearly Lectureship in "State Medicine and Public Hygiene."

Mrs. FitzPatrick, who in 1901 gave to the College under the advice of Dr. Norman Moore a sum of £2,000 to found a Lectureship in "The History of Medicine," in memory of her husband, Dr. Thomas FitzPatrick, a learned Member of the College.

Also the following donors of sums for the purpose of providing commemorative prizes or medals to be awarded by the President and Council of the College.

Dr. Swiney, 1844, jointly with the Society of Arts, a triennial prize of a silver cup value £100 for the best work on Jurisprudence.

Dr. Baly, 1866, £400 to provide a gold medal every alternate year for distinction in Physiology, "In Memoriam Gulielmi Baly, M.D.," and not restricted to British subjects.

A sum of over £400 subscribed in memory of Dr. Walter Moxon in 1886, the interest of which provides every third year a gold medal, value £30, for excellence in observation and research in Clinical Medicine, and is not confined to Fellows or Members of the College.

In 1895 Sir Hermann Weber generously presented to the College £3,000 to found a Prize to be called the "Weber Parkes prize," in memory of the late Dr. Edmund Parkes, to be awarded for the best essay on "The Pathology, Prevention or Treatment of Tuberculosis."

And in 1896, due to the suggestion of our Fellow, Dr. Theodore Williams, a sum of £1,000 was presented to the College by Captain Edward Wilmot Williams with the object of perpetuating the memory of the late Dr. Francis Bisset Hawkins, a former distinguished Fellow of the College. A gold medal is triennially

awarded to a Medical Practitioner who has advanced Sanitary Science or Public Health.

Turning now to those whose achievements have enriched not only our College by their reflected lustre, but mankind at large by the benefit conferred, and who by their intellectual labours and scientific results have gained for themselves imperishable fame, it would be wearisome and profitless to mention them merely by name. Time does not allow me to record their doings. Yet it were not decent that in the press and rush of the present day, the labours of Gilbert and of Glisson, of Willis, of Young who enunciated the undulatory theory of Light, of Sydenham, of Heberden, of William Hunter, of Prout, Bright, Watson, Parkes, Jenner, Gull, Clark, and Reynolds, former Fellows of this College, should be forgotten on this occasion of the commemoration of our benefactors. Excepting such immortal discoveries as Harvey's—discoveries that mark an era and are starting points in knowledge—it is easy from our present standpoint to overlook the help that the advances made by these workers contributed to the general progress; nor are those suggestions which proved to be erroneous altogether to be disregarded, since in their refutation the right way often became manifest.

These are but some of those whose good deeds,

whether in furthering the material prosperity of the College or adding to its reputation, deserve to be remembered as our benefactors. To commemorate, however, all such "by name and what in particular they have done for the benefit of the College," as I am enjoined to do by Harvey, would be beyond my powers, as I fear it would exceed your inclination to listen. But I should ill perform my duty in this connection did I not "exhort others to imitate these benefactors, and to contribute their endeavours for the advancement of the Society."

In attempting to comprehend the full significance of Harvey's great work on the circulation of the blood, it must be recollected that he was first and foremost an anatomist, and that although his discovery and its proof were the result of observation of the actual movements of the heart and vessels and of experiment on the living animal, it was by his previous knowledge of anatomy that he was enabled clearly to understand what he observed, and the perfection and, indeed, in great measure, the feasibility of his experiments, depended on his acquaintance with the structure of the organism he was investigating. And although the name of Harvey is for ever linked with a great and far-reaching physiological truth, one not only great in itself,

but if possible greater in being the starting-point of physiology and as such of a scientific pathology, it was from the anatomist's point of view that he came to enter upon the inquiry which was so fruitful in result. That a sound physiology is essentially dependent upon an accurate knowledge of anatomy was as well understood by Harvey as it is at the present day. "No one," said he, "indeed has ever rightly ascertained the use or function of a part who has not examined its structure, situation, connection by means of vessels, and other accidents in various animals, and carefully weighed and considered all he has seen."

It is further to be remembered that in the early part of the seventeenth century, anatomy was a science that, at least so far as the human body was concerned, had reached a very considerable degree of advancement. The teaching of Galen, who first properly appreciated the importance of physiology, full of error as it was, that had held sway for 1,400 years, was giving way to the more accurate work of Vesalius. His book, *Fabrica Humani Corporis*, published in 1543, and his teaching at Padua, marked the beginning of a new era in biological science, furnishing the account of the structure of the body as a basis upon which a precise physiology was alone possible, and from which a rational pathology and medicine could alone develop. The true

course being thus entered upon, the work was continued by the contemporaries, pupils and successors of Vesalius, chief amongst whom were Servetus, Realdus Columbus, Fallopius, Cæsalpinus and Fabricius—the last named being professor at Padua, whose instruction Harvey himself followed for nearly four years after quitting Cambridge in 1598. There, whilst perfecting himself in anatomy, he became more fully acquainted with such views as were held upon the circulation, modifications for the most part of the Galenic doctrine, with such objections thereto and glimpses of the truth as had been foreshadowed by Vesalius, and still more even by Servetus, who seems at least to have had some idea of the real nature of the pulmonary circulation.

For the first few years after his return to London Harvey appeared to have pursued his anatomical work, and especially the dissection of animals. Becoming a Fellow of this College in 1607 and in the next year a Physician to St. Bartholomew's Hospital, he was in 1615 appointed Lumleian Lecturer, the fourth holder of the office. The subject of the Lectures was Surgery, supplemented by public dissections so arranged as to form series of courses extending over six years. But it was as an anatomist that Harvey evidently regarded him-

self, for in the dedication of his work *De Motu Cordis et Sanguinis*, in 1628, "to his very dear friend, Doctor Argent, the excellent and accomplished President of the Royal College of Physicians, and to other learned Physicians, his most esteemed colleagues," he concludes thus, "Farewell, most worthy Doctors, and think kindly of your Anatomist, William Harvey." In the course of the Introduction to the same work, the author appeals to the similarity of structure of the two ventricles as being in favour of their pursuing a similar function, which was contrary to prevailing ideas. And, lastly, the final chapter of his "anatomical disquisition" shows how "the motion and circulation of the blood are confirmed from the particulars apparent in the structure of the heart, and from those things which dissection unfolds."

"Harvey's method of enquiry was that which may be called the purely and strictly physiological method. Observing carefully the phenomena of the living body, he sought in the first place, in the arrangements of the structures concerned in the facts of anatomy, for suggestions as to how the phenomena might be explained. It is this aspect of his method which brings into striking light the value of the work of Vesalius, and of the school of Vesalius, as the necessary preparation for Harvey's labours.

Vesalius opened up the way for Physiological enquiry by his exact anatomical labours . . . and his successors did little more than widen the way which he had opened up. Harvey was the first who followed up the anatomical path till it led to a great physiological truth. . . . He made no appeal to any knowledge or to any conceptions outside the facts of anatomy and the results of experiments. . . . The patient examination of anatomical features, if possible a comparison of those features in the same organ or part in more animals than one, the laying hold of some explanation of the purpose of those features suggested by the features themselves, and the devising of experiments by vivisection or otherwise which should test the validity of that explanation, that was Harvey's threefold method."[1]

I propose to consider how far the ascertainment of the facts of structure as a necessary preliminary or adjunct to experimental methods has influenced the progress of biological knowledge, and what may be the limitations and extensions of the subject in that direction. But I would guard myself at the same time from being supposed to assume that anatomy however

[1] Lectures on the History of Physiology during the Sixteenth, Seventeenth and Eighteenth Centuries, by Sir M. Foster, K.C.B., M.D., D.C.L., Sec. R.S., 1901.

complete and precise is of itself sufficient to furnish a physiology or knowledge of function, since in the earlier days to which I have referred, when the facts of gross anatomy were well and widely known, most erroneous and grotesque views were held as to the action and uses of the various organs—as indeed the history of the explanations offered for the circulation of the blood before Harvey's time sufficiently testifies. Nor do I fail to admit that much physiology, more especially in recent times, has become known even with considerable approach to exactness without a corresponding knowledge of its structural basis.

The discovery and proof of the circulation, not only by the actual positive knowledge which it furnished but also by the methods by which it was arrived at, did much to dispel the fanciful and absurd views held as to the other functions of the body, and thus became the starting point of Modern Physiology. But, as I propose to show by a few examples, the full benefit of Harvey's work, and indeed its applicability in explaining those other functions, was proportional to the extent of the accurate information possessed in respect to the structure of the organs by which these processes are carried out; and as fresh anatomical knowledge was forthcoming so did truer conceptions of the living activities develop

from their observation and experimental investigation.

The rational study of the *respiratory function*, for example, followed much the same course as did that of the circulation of the blood. The Galenic doctrine on this subject was that which Fabricius propounded in the Lectures to which Harvey listened. The air which was introduced into the lungs by the "rough artery," or, as we call it, trachea, was for the purpose of modifying and regulating the innate heat of the heart, of getting rid of the fuliginous vapours which resulted from this innate heat, and further with the object of being conveyed by the "vein-like artery" (pulmonary vein) to the left side of the heart, there to generate those vital spirits which the arteries carried throughout the body. Although Harvey's discovery set aside at once and for ever such unintelligible nonsense, as it had upset the corresponding views of the heart's action, the time was not yet quite ripe for laying the foundation of sounder principles as to the Function of Respiration. The state of knowledge of the anatomy of the lungs was scarcely as far developed in Harvey's time as was that of the structure of the heart, as may be seen in the *Mannuall of the Anatomy or Dissection of the Body of Man* (enlarged 1642) by Alexander Read, who was a Fellow of this College as well as "Brother

of the Worshipful Company of the Barber Chirurgions."

Moreover, the significance of the movements of the chest in breathing and of their relation to the entrance and exit of the air from the lungs was but imperfectly realised. If to Borelli may be attributed the first settlement of these questions on right lines, applying as he did to the problem the growing mechanical and chemical knowledge of his day, and showing as he did that the air entered the lungs as a result of atmospheric pressure as the chest enlarged by muscular contraction, and further that the air inspired was actually taken up by the blood, and that this was essential to the life of the animal, all of which are the facts which are at the basis of our present knowledge of the subject—if, as I say, Borelli showed this, the way for his so doing was made clear for him by what had been done in explaining the true structure of muscular tissues and still more by the labours of his fellow Professor at Pisa, Malpighi. More than fifty years after the invention of the compound microscope, this observer applied it to the investigation of the tissues, and had himself informed Borelli of the minute structure of the lungs, how the terminal branches of the air tubes ended in closed vesicles, on the walls of which the smallest blood vessels ramified, forming the

communication between the arteries and veins, thus supplying the completing link to Harvey's work within a few years after our great countryman's death. On such a foundation of exact knowledge the further pursuance of the subject was one for the Physicist and Chemist, and by them it has been brought to a high pitch, when once the nature of the machine they had to deal with was clearly defined. Observation and experiment were profitably occupied when anatomy had cleared the ground.

It was not only with the functions of respiration that Malpighi's great work on the investigation of tissue structure was concerned. His labours added to, and developed by the famous Dutchmen, Van Leeuenhoek, and Swammerdam, as well as by the Englishman, Robert Hooke, rendered possible the satisfactory examination of the other functions, though the difficulties of the necessary observation and experiment were greater than they were in respect to the circulation and respiration. Connected with the latter, questions of a mechanical and chemical character arose more capable of being answered as the sciences of physic and chemistry were at the time being better understood. While it was comparatively easy to see the movements of the heart and of the lungs in a living animal when these organs

were exposed, vivisection did not render so clear the secretory activities of the glands, the changes brought about by digestion, the intricacies of tissue nutrition, and the workings of the nervous system. These physiological problems were more intimately connected with the living material, and were not so open to such observation or experimental enquiry as was then possible. And it may be said that less even was known of the structure of the organs concerned in these functions than there was of the circulatory and respiratory systems. But, Malpighi and his fellows showed the way to what was wanted, and, as will appear, the uses and workings of the structures became open to scientific enquiry with important consequences.

Mention must be made however in further illustration of my theme of what had been done to provide truer conceptions of the nature of *muscular action.* Up to the time of Vesalius and even for some time afterwards the contractile power of the muscles was regarded as resident in the connective tissue sheath of the fibres, the true muscle substances being looked upon as packing. Vesalius first indicated the proper rôle of this material, but it was not until more than a century later, in 1664, that Nicolas Stensen a Dane, described (1664–1667) the structure of muscular tissue as he had studied it with the

microscope, and thus furnished Borelli with the anatomical grounds upon which he formulated the principles of the action of this substance, also showing that the contractions were induced through the nerves. Although much that he taught was erroneous, being entirely dominated by mechanical conceptions of the nature of muscular contractility, the refutation of his errors showed the right lines, which was possible only on the structural basis which the microscope had supplied.

The most important step towards an understanding of the nature of muscular action was taken shortly afterwards by a Fellow of this College, Francis Glisson, whose name is more generally associated with his work on the liver and on rickets. In a treatise however published by him in 1673 *De natura substantiæ energetica* he first explained the property of muscle substance, which he called "irritability," the character and phenomena of which were further developed by Haller nearly a century later.

Important as the work was that Malpighi did in respect to the discovery of the capillary blood vessels and the structure of the lungs, almost if not quite as great was that which he accomplished in connection with the *secreting glands*. In earlier times the word "gland" had a wider range of meaning and included such organs as

the brain and tongue. That some of them were concerned in straining off certain serosities or humours from the blood was also generally held, but the active agents in the process were quite unknown, as would naturally be the case when the intimate structure of the organs was hidden from view. To the nerves this function was often attributed, but the physiology of secretion was on a par with that of the heart or the lungs, with which of course it was interwoven. Within a few years previously to the time that Malpighi was investigating the structure of the skin, the liver, and the kidneys, the ducts of the pancreas and of the sub-maxillary and parotid glands were discovered and the position of these organs as secreting glands recognized. Malpighi in addition showed the lobular structure of the liver and the relation of the acini to the blood-vessels and showed the general course and arrangements of the renal tubules and of the glomeruli and capsules which were named after him. The instruments at his disposal did not permit him to realize the cellular constituents of these several organs, but all these researches gave the death blow to the older conceptions as to the part played by the nerves in the secretory processes, and it was realized that the secretions were derived from the blood in its passage through the glands and were passed into the commencements

of the ducts. Stensen indeed, who had discovered the sub-maxillary duct, seems to have had a foreshadowing of the vaso-motor influence on secretion.

The subject of *digestion* need not detain me, the problems with which it is concerned are so essentially chemical in nature, that although in the latter part of Harvey's period and within a few years after, the existence of the digestive juices as secretions of the various glands was known, their composition and mode of action on the food stuffs were not a matter of anatomical enquiry and depended for their satisfactory investigation upon a knowledge of chemistry that was developed altogether outside the study of the living body.

Of all the functions of the body, none it would be at once admitted is more difficult of study or more obscure in the investigation than that of the *nervous system.* None also has been the subject of cruder description or wilder explanations. That this is so would seem natural, clearly associated as nervous phenomena are with the more recondite phenomena of life, and subject as their study has ever been to the influences of metaphysical speculation. At the same time, however, one, and as I conceive the most, important circumstance that has retarded the rational development of Neurology has been the extreme

difficulty that has existed in obtaining a precise knowledge of the actual structure of the organs which subserve the function. Even at the present day, great as are the advances that have been made, I think I should be correct in saying that less is known of the minute anatomy of the nervous centres than of any other organs in the body. Whilst it may be said that with the present means at our command a knowledge of the histology of many of the tissues has reached or almost reached its limits, the precise disposition of the multiplicity of nerves and cells that constitute the brain and spinal cord as well as the ultimate termination of the nerve fibrils in the tissues and in the centres are yet to seek. Hence it is that whilst during the period of Harvey and immediately subsequent the study of the several functions to which I have referred began to emerge from the erroneous and fanciful notions by which they were surrounded, and their investigation to be started upon lines that have been followed to the present day, the phenomena of the nervous system for some time remained enveloped in the mystic obscurity that had enshrouded them with growing intensity from the earliest times, an obscurity that was if possible made greater by the lengthy and unintelligible phrases in which they were described.

The main divisions of the brain were re-

cognised—cerebrum and cerebellum, medulla, corpus callosum, corpora striata, ventricles, and even such smaller parts as the corpora geniculata, pituitary body, pineal gland, infundibulum and septum lucidum. The distinction into white and grey matter was also appreciated, and that the former was made up of fibres was supposed. But the structural relation of the several parts to one another was very indistinctly realised, and anything further as to minute structure was of course unknown. The following extract from Read's *Mannuall of Anatomy*, already mentioned, will serve to show what was taught in a standard text book, as we should now call it, concerning the functions of the brain.

> "Of the action of the brain. The action of the brain is this: After that the spirits and blood are discharged into the Sinus of the dura mater by the veines and arteries to temper the heat of them, the brain is ordained (seeing it is colder than the heart) that the animall functions, which are feeling and moving maybe the more readily executed. Wherefore the animall spirits seem not to differ from the vitall spirits in substance, but in qualities, viz. the temperament and attenuation; for they must be more temperate because heat doth both taint the

reasons (as we may see in drunkenness and raving) and hindereth or preventeth the motion.

The spirits ought also to be more subtill; because they are to passe like a thunder through the bodies of the nerves. So, as the vitall spirits are carried to the parts of the bodies by the arteries, so the animall are carried by the nerves.

The animall spirits for this cause also ought to be subtill because the reasonable soul is resident in the brain, which doth contemplate things immateriall, as Angels and it selfe."

Although this may be taken as illustrating the teaching of the day, and the work of Willis, *De Cerebri Anatome* (1659) was no advance thereon, there appeared ten years later (1669) a treatise on the anatomy of the brain by Nicolas Stensen, whose investigations on secretions have already been referred to, which foreshadowed in several respects many of the discoveries made and views held a century and more later. But the special feature of this treatise as bearing on my present subject is that after pointing out the extremely slight information possessed as to the essential structures of the nervous system Stensen refused to admit in face of the lack of all sound anato-

mical knowledge any physiological deductions whatever. After pointing out a number of cases in which he shows that adequate anatomical knowledge is wanting, he says, "whence you may guess how little trust is to be put in explanations based on such a futile foundation. . . . I have said nothing of the use of parts, nothing of the actions which we call animal, since it is impossible to explain the movements carried out by a machine, so long as we remain ignorant of the structure of its parts."[1]

It was only very gradually that this ignorance was cleared up, and pending that the progress of Neurology was hindered by the mystical speculations of successive metaphysical doctrines. Until the discovery of the nerve cells and their connection with the fibres was made in the fourth decade of the last century, no very sound notions on nervous function were possible.

Slowly, and with many throw-backs as Physiology emerged from obscurity and became established on a scientific basis, the correlative subject of Pathology long lagged behind in taking up a similar position. The reluctance, as it were, to regard morbid processes in the same manner as those manifested by a healthy organism was no doubt in part responsible for the delay in estab-

[1] Foster, loc. cit. p. 280.

lishing any clear conceptions as to the nature of disease; but what was of far greater influence was the lack of any systematic observations on the appearances presented after death, anything that in short might be looked upon as a knowledge of morbid anatomy. Hence it was that the notions respecting disease and its workings were, if possible, more fanciful than those held in connection with the healthy body and were retained for long after the latter had been diverted on to right lines. Although isolated records of post-mortem examinations were made from time to time,[1] it was not until the latter half of the eighteenth century that Morgagni laid the foundations of a scientific Pathology by his work *De Sedibus et Causis morborum* (1780), upon which a goodly superstructure was soon erected. "It stands most clearly revealed," says Virchow,"[2] in the history of Pathology that the division of the body first into the larger regions (head, breast, abdomen, etc.), then into organs, then into tissues, and finally into cells and cell territories, was the first step which opened up to us the comprehension of disease." There is good ground for thinking that this comprehension

[1] Bonetus in 1675 had collected many such in his *Sepulchretum.*

[2] Huxley lecture delivered at Charing Cross Hospital, October 3rd, 1898.

would have been sooner grasped if the collection of records of post-mortem inspections which Harvey had made over many years had escaped the destruction which befell many of his papers when his house in London was ransacked during his absence at Oxford with King Charles.

From this necessarily brief sketch of the development of Physiology in relation to gross anatomy, it is apparent that any approach to an accurate understanding of the working of the several functions was only possible when the facts of structure were ascertained and appreciated; and that when these facts were scanty, fanciful and erroneous views were entertained as to the mode in which the corresponding functions were performed. With the prosecution of anatomical investigation beyond the range of the unaided vision, the knowledge of the living organism and its working was by so much extended. The impetus given to the study of microscopic structure by the labours of Malpighi and his followers resulted in establishing Histology—a term first used by Carl Meyer in 1820—as a specific branch of anatomical enquiry which has been pursued with ever growing success to the present day, coincidently as the realm of gross anatomy became more and more restricted.

so far as the human organism is concerned, by the very completeness of the knowledge of it.

Did time permit it would not be difficult to show, as indeed has in part already been done, that as the intimate structure of the tissues became revealed, so was the way cleared for a sounder Physiology, whether by a more rational understanding of what observation and experiment had disclosed, or by indicating the direction in which observation and experiment might be fruitfully continued.[1]

The progress of histological research has been marked by one great feature far-reaching in its effects and of fundamental importance—the recognition of the cell as the tissue unit. And whilst this has served to give completeness to the views of tissue structure, and especially of

[1] In the record of histological advance mention must be made of Haller's *Elementa Physiologiæ* (1757), which first presented in a collected form the most correct information on the structure of the tissues; and of Bichat's *Anatomie Generale* (1801), a still more comprehensive work on the same lines in which also the foreshadowing of the cellular nature of the tissues, established by Schwann in 1838, was faintly indicated, although the word "cell" was not used. To the labours of John Goodsir and still more to the epoch-making work of Virchow on Cellular Pathology (1858), must be ascribed the demonstration of the fact that our conceptions of morbid processes must be founded on a histological basis, a doctrine that the great Pathologist's *Archives für Pathologische Anatomie und Physiologie und für Klinische Medizin*, has for nearly half a century consistently expounded.

the genetic relations of the several components of the organs, it served as a fresh starting point for the investigation of biological problems, since it was recognised that the life of the organism was but the life of the cell, differing only in degree of completeness; and the full realisation of what is meant by specialisation of function being dependent on differentiation of structure became apparent.

With entire appreciation of the complexity of the phenomena presented by the living cell, Brücke forty years ago affirmed that this must imply the existence of some structural arrangement in the cell substance, some degree of organisation, some further stage than had hitherto been detected in the disposition of the material which subserves function. Setting aside the obvious distinction into cell-contents nucleus and attraction-sphere, and the separation of the first into cell-protoplasm and the metabolic products thereof (such as starch, fat, glycogen and pigment granules), attention has been directed towards discovering in the apparently homogeneous protoplasm some evidence of structure. Numerous observers long ago described a fibrillar arrangement in this material, a view that later gave place to the assertion that it is rather to be regarded as of a reticular nature, the protoplasm forming a network or meshwork "the nodal

points of which appear as individual granules."[1] The imperfect loculi formed by this disposition of fibrils were described as being occupied by a more fluid material. Objections were raised to this explanation, and the more recent description with which the name of Bütschli is associated attributes to the protoplasm a "foam-structure, which depends upon the presence within a uniform ground mass of a large number of extremely fine vacuoles lying almost at the limit of microscopic visibility, and so close together that their walls consist of relatively thin lamellæ." (Verworn.)

But these and several other views as to the intimate structure of the living cell-protoplasm which describe it "as being composed of two substances, one of which is disposed as a contractile net according to some, as a relatively rigid framework according to others, or as free filaments; or whether it be built up of a more solid material and of a more fluid material which occupies the minute spaces or vacuoles which are hollowed out in the former,"[2] have not met with universal acceptance, and there are still those who regard these relatively coarse indica-

[1] *General Physiology* by Prof. Max Verworn, translated by F. S. Lee, Ph.D., 1899.

[2] "The Structure of Cell-Protoplasm," by W. B. Hardy. *Journal of Physiology*, 1899, Vol. XXIV., p. 159.

tions of structure in cells as the results of post mortem change or of fixing reagents. To such the living protoplasm is a homogeneous colloid, and its "peculiar and transcendental qualities are associated with molecular rather than with molar structure."

What is said as to the cell-contents applies also to the nucleus in which a reticular or mesh-work appearance is described by some, as others would regard the actual living condition as one of perfect homogeneity.

In this uncertain state the question of the intimate structure of the living cell-protoplasm must at present be left, so far as the same is capable of investigation by the microscope and its accessories. But, whilst fully recognising that with further improvements in method and in means this problem will be solved, it none the less seems certain, consistently with the present hypotheses as to the nature of the cosmos, that however far the eye may be able to penetrate, there will still remain behind and beyond a molecular or atomic structure, for the understanding of which other branches of scientific enquiry must be employed. "The organism," says Virchow, "is not an individual but a social mechanism. An exact anatomical analysis of this mechanism always brings us at last to cells; they are the ultimate constituents of all tissues

as they were their origins. Hence we call them the living elements, and hence we regard them as the anatomical basis of all biological analysis, whether it has a physiological or a pathological object in view. The cells are composed of organic chemical substances, which are not themselves alive, but the mechanical arrangement of which determines the direction and power of their activity." [1]

Before proceeding to consider the next and chemical stage of structure it would be well shortly to indicate some of the living phenomena which have either already received or still await their explanation in the intimate histology of the cell. Most important of these is contractility, whether this be manifested as irregular amœboid movements, the rhythmic wavings of cilia, or the orderly and more highly differentiated contraction of muscular tissue. The ebb and flow of the more diffluent portions into and out of the reticulum of the spongio-plasm—the "streaming" as it has been termed—is a step towards explaining—apart altogether from the attempt to express protoplasmic movement in terms of inorganic phenomena, such as has been done—those alternate contractions and expansions of the bioplasm due to reciprocal rearrangements of its particles, which constitute one of the

[1] Huxley Lecture.

most striking characteristics of the living organism.

The complicated changes connected with nuclear division, known as 'mitosis,' which underlie all cell-multiplication, and hence are of such importance in growth and development, are only realised as the result of those microscopic investigations which have been directed towards discovering a structural organisation of the cell itself.

The germ-plasm and the problems of heredity connected therewith can only be discussed in terms of cell structure with any probability of satisfactory results.

Possibly also the varied morphological characters presented by the fully developed living constituents of the tissues, developed as they have been through successive stages from cells of almost identical appearance in the blastoderm, may be more fully understood when the structure of their protoplasm is more accurately known. The so-called specificity of cells and its limitations—metaplastic interchanges—so important in the study of tumour formation, is an aspect of this same question.

From the earliest recorded times there has prevailed an idea which ascribed to matter an ultimate composition of indivisible indestruct-

ible particles or atoms, and by no one was the atomic theory more firmly maintained than by the contemporary of the later years of Harvey—Isaac Newton. "To me," said he, "it seems probable that God in the beginning formed matter in solid, massy, hard impenetrable particles of such sizes and figures, and with such other properties and in such proportion as most conduced to the end for which He formed them." By Robert Boyle also, to whom Natural Philosophy in the seventeenth century owed much, the theory was held, though he found the explanation of chemical changes in the differences of atomic structure and arrangement of one single form of matter rather than of different elements—a crude foreshadowing of the present day conception by Sir William Crookes of the fundamental matter or "protyle." As is well known, however, it was not until the early years of the last century that the atomic theory received its practical development by John Dalton, since when it has remained at the foundation of physical and chemical science. "Despite attacks and criticisms," says Prof. Clarke in his recent Wilde Lecture,[1] "Dalton's generalisation still holds the field; and from it, as from a parent

[1] Delivered May 19th, 1903, to the Manchester Literary and Philosophical Society, on the occasion of the Dalton Centenary Celebrations.

stem, spring nearly all the other accepted theories of chemistry." The conception of an atom as the smallest conceivable portion into which an element can be divided, or that can enter into combination, and attaching to the idea of the atom a definite relative weight constant for atoms of the same element but differing with different elements, gave a satisfactory explanation for the laws of definite proportions and of multiple proportions which previously had been but incompletely recognised.

Since, with the exception of a few elementary gases, an atom is always combined with one or more atoms of the same or of other elements, some term is required to denote the smallest portion of the substance capable of a separate existence, and for this the word "molecule" is employed. Built up on these fundamental ideas there has developed among other great generalisations the chemistry of the carbon compounds, and the hypothetical recognition of the relative arrangements of the atoms within the molecule—in short, chemical constitution or chemical structure. Now "the greater the valency of an element the more complicated are its combining ratios and the greater the possibility of its atoms forming numerous compounds with similar and dissimilar atoms." The atoms of carbon, which is the chief element in so-called organic bodies,

"possess, in a much greater degree than those of any other element, the property of combining with similar atoms whereby a part of their valencies are satisfied."[1] Thus may be formed groups of carbon atoms so linked together that their valencies are in part satisfied among themselves, constituting what are known as carbon nuclei; and the free valencies being satisfied by atoms of other elements molecules are formed in which much energy is accumulated with more or less instability.

Such conceptions as to the fundamental nature of matter, of its molecular structure and arrangement of atoms therein, permitted the laying down of rational or structural formulæ for chemical compounds; and when the further suggestion was made of linking the atoms in tridimensional space rather than in a single plane a still further extension of the idea of the atomic disposition within the molecule became possible and "stereo-chemistry was born" (Clarke).

With theories of this kind ready at hand, theories which had done and are still doing so much to explain the phenomena met with in the domains of physics and chemistry, what more natural than that the biologist, recognising that but little progress was being made in the

[1] *Principles of General Organic Chemistry*, Prof. Hjelt, 1890.

further investigation of the intimate cell structure, and that the histologist in point of fact was apparently at the limit of his range of observation, should turn to the physico-chemical sciences for the satisfaction of his quest?

The attempts, however, from the chemical side to explain the constitution of living matter, due no doubt to the extreme complexity of the subject, cannot be said as yet to have led to any very definite result, although several very suggestive hypotheses have been put forward. Inasmuch as no empirical formula has up to the present been constructed for any one of the typical proteids, a rational or structural formula for the constitution of the undoubtedly large molecules of which these substances are composed is scarcely to be expected; and this, although necessary, is but only the first stage in the inquiry. Analysis of proteid bodies gives rise to numerous products; the end substances such as carbonic acid, water and urea we are familiar with, but the intermediate ones "fall into two principal groups, the fatty compounds (generally containing an amidogen radicle) and the aromatic compounds or derivatives of benzene."[1] Accurate as further work in this direction may become however, it still of neces-

[1] Prof. Halliburton, M.D., F.R.S., in Prof. Schäfer's *Text Book of Physiology*, Vol. I., p. 35.

sity will not be a satisfactory explanation of the composition of living protoplasm which is ever in a state of flux; the continuous decompositions and reconstructions of which underlie its activities, are indeed phases of its living. Hitherto all attempts to ascertain the composition of the bioplasm have resulted in killing the material, and hence the solution of the question is evaded. And although proteids are obtained from living protoplasm, there is no proof that they exist as such in the living matter, but rather are they the dead derivatives of what is killed in the process of examination.

Theories of the constitution of proteids arrived at by the attempted synthesis of these substances have been provisionally set out, and though no one of them is free from objection, it may reasonably be supposed that consistent with prevailing chemical theories, they are on the right lines. Among these should be mentioned that propounded by our distinguished Fellow, Dr. P. W. Latham, according to which what he terms "living proteid" is composed of a chain of cyan-alcohols and a thio-alcohol united to a benzene nucleus. These cyan-alcohols are exceedingly unstable and prone to undergo intramolecular changes, properties also possessed in a marked degree by bioplasm, and similar bodies are obtained from the disintegration of both cyan-

alcohols and proteids. A more recent attempt in the same direction has been made by Verworn,[1] who describes the "Biogens," as he terms them, as real chemical and physical entities, each consisting of a benzene nucleus, round which are arranged various groups of atoms, the idea being arrived at by a study of the metabolic products of the organism. The extreme lability of the biogen Verworn attributes to the incorporation of oxygen in the molecule, the absence of which rather than the accumulation of waste products he regards as responsible for the cessation of the irritability of the bioplasm. It may be further observed that it is in the cell-protoplasm, and not in the nucleus that this observer locates the biogens.

Whilst fully realising the purely speculative character of these conceptions, the provisional use that they may be in comprehending the activities of the living organism is apparent. For many of these complex processes the knowledge of the chemical anatomy of bioplasm is as essential as the gross anatomy of the organs concerned is for an understanding of the circulation of the blood. As the chemist and the physicist find in the atomic theory and its developments an explanation of the properties or functions of

[1] *Die Biogen-hypothese*, 1903. See also *Nature*, Feb. 26th, 1903.

the non-living bodies with which they deal, so may the physiologist find in the same assumptions a clue to those even more abstruse functions displayed by living materials and furnish to the pathologist and to the clinical physician those data upon which a fuller realization of morbid processes may be obtained, and sounder principles for their prevention or their treatment be laid down.

Where in the whole range of physiological enquiry is to be found a region into which the observer has less penetrated, and where for want of some guidance he is more adrift in the comprehension of what he does recognize than in the complicated region of "nutrition?" And yet how essential for the mere framing of a proper dietary, or for an understanding of the protean symptoms collectively denominated "gout" is it that we should be able to form some idea of what becomes of the absorbed food stuffs, when having undergone some elaboration in the epithelial cells, the hepatic tissues, and the blood through which they have passed, they come "within the sphere of influence" of the living cell. What too is more important than to be able to attach to the comprehensive term "metabolism" some rational meaning, based upon a knowledge of what actually occurs, and what structural arrangements and rearrangements

take place within the bioplasm of the tissues? The practical importance of this must be obvious and should prevent the relegation of such questions as I have been discussing as transcendental and of no useful purpose.

One of the greatest and most far-reaching advances in Pathology within recent years is undoubtedly the recognition of the part played by micro-organisms in the causation of disease. But the full value of the knowledge gained is not comprised in the detection and cultivation of the specific bacillus or yet even in the discovery of the particular toxin which the microbe produces, important as such information is. We require to know how and why these poisons affect the tissues as they do; and in order to arrive at that the rational formulae of these poisons must be known, and what is more the molecular structure of the living cells upon which the noxious material acts, ere we can realize how by some untoward substitution in the atomic arrangement of the living molecule its activities are prejudicially affected.

Our treatment of disease by drugs has been forcibly if irrevently described as "pouring substances of which we know little into bodies of which we know less." Pharmacology has done a little towards removing this reproach and that department of it which deals with the

relation between the chemical composition and constitution of a substance and its physiological action no doubt lies at the root of all rational drug therapeutics. But no one can assert that so far very much is known in this direction or that there has been much practical outcome of the investigations. This would be quite otherwise however if we had a knowledge of the molecular structure of living matter which would show the perversions taking place in disease and indicate the way in which they could be corrected.

The subject of immunity may reasonably be expected to find its interpretation in the ultimate constitution of the tissue elements ; as also those at present vague conditions which we are dimly conscious of, represented by such terms as "bodily constitution" and "temperament." So too the differences in response on the part of different individuals to the same morbific influence, the variations in the manifestation of what we speak of as the same disease in various persons that lead the sagacious physician to treat the patient and not the malady ; and in brief those intangible characters which determine the responsibility of the organism for morbid symptoms, as distinct from the injurious agent that we speak of as cause, each and all await their explanation. The factors of the environment which condition the vitality of the tissues are not to be found solely

in such external conditions as are commonly comprised in that expression.[1] The behaviour of cell to cell, their mutual interactions—cytotaxis—and their physiological resistances the one to another will have to be taken into account in forming any thorough conception of the totality of life whether healthy or diseased that an organism presents, and the understanding of such problems cannot be attained until the finite structure of the material concerned be rendered plain, or be assumed with such justification as those concepts underlying physico-chemical action at present furnish.[2]

[1] As illustrating these environmental relationships may be mentioned the various forms of taxis or tropism whereby the direction of the movements exhibited by living protoplasm may be influenced. The best known of these is "chemotaxis" as met with in connection with some states of leucocytosis, but it is probable that other forms of taxis caused by pressure, gravity, heat and light also prevail. "The spermatozoon seeks the ovum, and almost everywhere in the living world is led in the right path by the chemotactic action which the metabolic products of the egg-cell exert upon the freely moving sperm-cell. . . . Every species of spermatozoon is chemotactic to the specific substances that characterise the ovum of the correspond-species" (Verworn, *General Physiology*). The effect also of these external agencies, as well as others like moisture and the density of the surrounding medium on the nutritional activity and on the power of reproduction as well as on the motility of the simplest organism has been experimentally demonstrated; suggesting a chemical complexity of protoplasmic structure which is open to disturbance by the external world.

[2] As bearing upon these and like questions a very large body of experimental evidence exists to show that there are great

May it not also be in this same molecular structure of living matter that will be found the explanation of those phenomena of development and of organic evolution by which the fertilized ovum of two different kinds placed under identical conditions will each attain "to such form and structure as best fit it for its place in nature —processes which cannot be measured or ob-

varieties in protoplasm, and that it is far from being of a uniform character in all cases as was formerly supposed, and this even amongst the simplest of unicellular organisms. The differences in behaviour exhibited by different species in response to various reagents clearly demonstrate this. For whilst some are so profoundly affected in their molecular constitution as to succumb on being subjected to certain poisons, others, in no wise differing so far as can be determined by the means at present at our disposal, are quite uninjured. Some kinds of bioplasm appear to have a general high resistance to all chemical agents, while others have a high or low resistance to particular agents only; thus nervous tissue for instance is readily and injuriously affected by substances (*e.g.* cocaine or nicotine) to which many protophyta are indifferent. Since also many toxic bodies which produce no effect upon dead albumen are yet violently poisonous to living protoplasm, it would seem probable that the latter contains in its construction certain unstable groups of molecules which undergo replacement by others from toxic agents. All this goes to show that protoplasm is extremely complex and consists of numerous kinds of compounds, many of which are very unstable. Also that not all protoplasm contains the same compounds but that these are dissimilar in different organisms. And further that not all of the compounds in any protoplasmic body are essential to life, and that we may so act on a protoplasmic body by a weak reagent and gradually change its composition, so that it will no longer be killed by a strong solution of the same reagent, thus effecting an acclimatization, or as we should say rendering the organism immune. (See *Experimental Morphology* by Dr. Davenport, 1897).

served by the same methods as are used in the investigation of the phenomena of non-living nature, *i.e.* by measurements of their time and place relations under varying conditions, in other words by the method of experiment,"[1] which are applicable to other processes of living organisms? "The biogen hypothesis gives a plausible account of growth and the production of fresh living material by supposing that the molecule is capable of polymerization (*i.e.* the union of a number of molecules to form a single molecule) and then of falling into simpler substances once more."[2]

Surely I need not plead for the importance of these questions I have set out, an importance that is not merely the concern of the biologist, but is that of the practical physician. In the solution of these problems lies widespread benefit to mankind.

Yet one step further. The atomic theory of the constitution of matter and its developments—although they have hitherto sufficed for the needs of the chemist who concerns himself with the decompositions and reconstructions of substances—is not the last word for some at least of

[1] Sir J. Burdon Sanderson, Bart., M.D., F.R.S., *The Times*, May 11th, 1903.
[2] *Nature*, loc. cit.

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the most progressive physicists. Is the atom indivisible and finite?—has ever been a question that even the most pronounced atomists have asked themselves from time to time, and if the explanation of the recent discoveries that have been made in connection with radio-activity be correct, the answer must be in the negative. Briefly to summarize from this year's Romanes Lecture by Sir Oliver Lodge, the most advanced views that physicists are inclined to hold, it may be said that the atom is conceived as consisting of an aggregate of what have been termed corpuscles, and further that each atom may have associated with it a definite charge of electricity, atoms of different kinds having multiples of this charge, such an electrically charged atom being termed an "ion." Now the smallest unit of electric charge which itself "possesses the most fundamental and characteristic property of matter, viz. mass or inertia," is known as an "electron," and the charge with which the atom is possessed consists of a number of these electrons. Within the atom the "electrons are in a state of vigorous motion among themselves." But it has been found that the electrons can be detached from the atom at an electrode, and such isolated particles form the cathode rays which when stopped suddenly by a massive obstacle give rise to the so-called

Röntgen rays. Hence the electron "is the most definite and fundamental and simple unit which we know of in nature." Whether, however, the electron is to be considered as solely consisting of electrical charge, or whether this be associated with a material particle is a moot point. Some hold that the latter is non-existent, and that in place of there being two kinds of inertia, which we speak of as material and electrical, the latter alone exists, the atom therefore being "composed solely of electricity." Such a concept of the electrical nature of matter is obviously a more precise expression of the monistic theory, in accord with which matter and energy are but convertible terms. Such a hypothesis suggests also that the various elements as we know them are but "different groupings of one fundamental constituent," the atom of each one consisting of its own special number of electrons; the unity of matter being thus arrived at.

Highly speculative as such considerations are, they nevertheless find support in electrical phenomena, and still further in radio-activity, of which we have heard so much in connection with radium and allied substances. This radio-activity "consists in the flinging away with great violence of actual atoms," which exceed in their rate of movement the fastest cannon-ball ever

projected. The substance left is also radio-active, and successive residues, differing as they do from each other, yet continue to exhibit radio-activity, "and one of the residues so left seems ultimately to pitch away electrons simply instead of atoms of matter"—a veritable transmutation of matter. Thus it is supposed that "the massive and extremely complex atoms of a radio-active substance are liable to get into an unstable condition . . . and gradually disintegrating fall into other and ultimately more stable forms of matter." Yet it appears that as the radio-active substance thus breaks up, fresh radio-active matter is as constantly regenerated, possibly, as Lord Kelvin has suggested, from the ethereal waves surrounding the atoms.

Even as the atomic and molecular theory was laid hold of to furnish an explanation of that flux of chemical activity which we denominate bioplasm, so have these further speculations on ionic action been pressed into the same service, and with some promise, wholly hypothetical as they may be. It is to Professor Loeb, of Chicago, that we in the main owe the application of the ionic theory to physiological phenomena. "The bulk of protoplasm," he writes,[1] "consists of colloidal material, and the physical manifestations of life, such as muscular contraction, protoplasmic

[1] *American Journal of Physiology*, 1901–2.

motions, and the innervations, are due to changes of the condition of these colloidal solutions. And the reason why the electrical current is the universal form of stimulation is that the particles in colloidal solutions are electrically charged, and that every alteration of the charge of the particles will result in a process of innervation, or a contraction, or a protoplasmic motion." Thus nerve action is simply electrical action, negative ions being released where nerve blends with muscle or where systems of concatenated neurons come into connection. Ion after ion is precipitated, and thus neural conduction takes place.[1] This play of ions is excited or inhibited by the character of the fluids with which the protoplasm is bathed, by the nature, that is, of the ions which these fluids contain. Most effective in stimulating protoplasmic action are such substances as sodium salts, as those of lime restrain it, and since such inorganic bodies are among the products of tissue waste, it may be that in the ions of metabolism are to be found the causes of that rhythmic tendency to activity which nerve cell and muscle fibre alike exhibit. If normal neuro-muscular action may be thus induced, the theory offers a clue to the comprehension of some of the most obscure morbid manifestations of these tissues, for, says Professor Loeb, "that

[1] *Philadelphia Medical Journal*, March 22, 1902.

certain ions are capable of bringing about forms of irritability in nerves and muscles which do not exist normally may perhaps furnish the explanation of a certain number of morbid phenomena (neurosis and hysteria) in which the motor and sensory reactions of the patient are modified."

In thus labouring as it may seem the successive phases of structure from the grossly obvious to such as the microscope discloses, and thence to the hypothetical chemical and electrical constitution of the material involved, it is not for a moment claimed that the investigation and the observation of functional manifestation have waited for anatomical discovery. In many departments of physiology, notably in that concerned with nerve and muscle and with secretion, a large mass of information has been acquired as the result of carefully devised experiments, whilst but little has been done towards ascertaining the ultimate structure of the tissues concerned, little, that is, beyond what was known a score of years ago or more. But in respect to such tissues as these, microscopic examination would seem almost to have reached its limits, and for the complete comprehension of the physico-chemical phenomena, more recently ascertained, the problem of the chemical and electrical constitution of

the muscle or nerve-fibre and of the gland-cell awaits solution. Though it may be true that "it is quite impossible to attain to a complete knowledge of function without a thorough anatomical analysis" (Huxley), and this it may be added although the observation of function may have led to the study of structure, yet it is clear that "structure" must include a wider range of meaning than hitherto it has been commonly thought to bear, and to reach into those regions where observation is conditioned by speculation and where theory has to take the place of demonstrable fact. However true it may be that for a general conception of the physics of the circulation Harvey was beholden to his anatomical knowledge, it is also true that for our later acquired information of the share taken in the movement of the blood by the arteries an acquaintance with the structure of these vessels is necessary, whereby their elasticity and their tone are referred each to its own tissue. The problem that Harvey solved was one that in its broad features was a mechanical one; but it does not end with such information as the gross anatomy of the organs and the histology of the tissues supply. Behind it lie the contraction of the muscular substance of the heart and arteries and the nervous governance of that material, which involve considerations of another character.

For the complete understanding of the electrical and chemical changes which are associated with the passage of the nervous stimulus and the muscular response we should require to know what are the underlying molecular rearrangements and alteration in chemical constitution crudely represented by the formation of certain waste products. At this point I say precise knowledge fails us, and we turn for assistance to theories which have been so helpful in the explanation of the properties of non-living matter, consistent therein with the principle laid down by Mayer half a century ago "to refer both vital and physical phenomena to a common measure."

Whilst I have endeavoured to illustrate with such completeness as my brief time permits the relationship of physiology to anatomy, whether normal or morbid, the general tenour of my remarks will have indicated that when the limits of visibility even with our most perfect instruments have been reached the separate investigation of structure and of function no longer becomes possible. The molecular constitution, chemical or electrical, of living matter becomes conceivable only in terms of action, and function and structure are but aspects of each other.

No deeper secrets of nature exist to be searched out by observation and experiment than

these, and none will more benefit mankind in their discovery. To their investigation therefore, in obedience to the precept of Harvey, do I exhort you to turn.

R. CLAY AND SONS, LTD. BREAD ST. HILL, E.C., AND BUNGAY, SUFFOLK.

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British Association for the Advancement of Science.

CAMBRIDGE, 1904.

ADDRESS

TO THE

ZOOLOGICAL SECTION

BY

WILLIAM BATESON, M.A., F.R.S.,

PRESIDENT OF THE SECTION.

In choosing a subject for this Address I have availed myself of the kindly usage which permits a sectional president to divert the attention of his hearers into those lines of inquiry which he himself is accustomed to pursue. Nevertheless, in taking the facts of breeding for my theme, I am sensible that this privilege is subjected to a certain strain.

Heredity—and variation too—are matters of which no naturalist likes to admit himself entirely careless. Everyone knows that, somewhere hidden among the phenomena denoted by these terms, there must be principles which, in ways untraced, are ordering the destinies of living things. Experiments in heredity have thus, as I am told, a universal fascination. All are willing to offer an outward deference to these studies. The limits of that homage, however, are soon reached, and, though all profess interest, few are impelled to make even the moderate mental effort needed to apprehend what has been already done. It is understood that heredity is an important mystery, and variation another mystery. The naturalist, the breeder, the horticulturist, the sociologist, the man of science and man of practice alike, has daily occasion to make and to act on assumptions as to heredity and variation, but many seem well content that such phenomena should remain for ever mysterious.

The position of these studies is unique. At once fashionable and neglected, nominally the central common ground of botany and zoology, of morphology and physiology, belonging specially to neither, this area is thinly tenanted. Now, since few have leisure for topics with which they cannot suppose themselves concerned, I am aware that, when I ask you in your familiar habitations to listen to tales of a no man's land, I must forego many of those supports by which a speaker may maintain his hold on the intellectual sympathy of an audience.

Those whose pursuits have led them far from their companions cannot be exempt from that differentiation which is the fate of isolated groups. The stock of common knowledge and common ideas grows smaller till the difficulty of intercommunication becomes extreme. Not only has our point of view changed, but our materials are unfamiliar, our methods of inquiry new, and even the results attained accord little with the common expectations of the day. In the progress of sciences we are used to be led from the known to the unknown, from the half-perceived to the proven, the expectation of one year becoming the certainty of the next. It will aid appreciation of the change coming over evolutionary science if it be realised that the new knowledge of heredity and variation rather replaces than extends current ideas on those subjects.

Convention requires that a president should declare all well in his science; but

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I cannot think it a symptom indicative of much health in our body that the task of assimilating the new knowledge has proved so difficult. An eminent foreign professor lately told me that he believed there were not half a dozen in his country conversant with what may be called Mendelism, though he added hopefully, 'I find these things interest my students more than my colleagues.' A professed biologist cannot afford to ignore a new life-history, the Okapi, or the other last new version of the old story; but phenomena which put new interpretations on the whole, facts witnessed continually by all who are working in these fields, he may conveniently disregard as matters of opinion. Had a discovery comparable in magnitude with that of Mendel been announced in physics or in chemistry, it would at once have been repeated and extended in every great scientific school throughout the world. We could come to a British Association audience to discuss the details of our subject—the polymorphism of extracted types, the physiological meaning of segregation, its applicability to the case of sex, the nature of non-segregable characters, and like problems with which we are now dealing—sure of finding sound and helpful criticism, nor would it be necessary on each occasion to begin with a popular presentation of the rudiments. This state of things in a progressive science has arisen, as I think, from a loss of touch with the main line of inquiry. The successes of descriptive zoology are so palpable and so attractive, that, not unnaturally, these which are the means of progress have been mistaken for the end. But now that the survey of terrestrial types by existing methods is happily approaching completion, we may hope that our science will return to its proper task, the detection of the fundamental nature of living things. I say *return*, because, in spite of that perfecting of the instruments of research characteristic of our time, and an extension of the area of scrutiny, the last generation was nearer the main quest. No one can study the history of biology without perceiving that in some essential respects the spirit of the naturalists of fifty years ago was truer in aim, and that their methods of inquiry were more direct and more fertile—so far, at least, as the problem of evolution is concerned—than those which have replaced them.

If we study the researches begun by Kölreuter and continued with great vigour till the middle of the sixties, we cannot fail to see that had the experiments he and his successors undertook been continued on the same lines, we should by now have advanced far into the unknown. More than this: if a knowledge of what those men actually accomplished had not passed away from the memory of our generation, we should now be able to appeal to an informed public mind, having some practical acquaintance with the phenomena, and possessing sufficient experience of these matters to recognise absurdity in statement and deduction, ready to provide that healthy atmosphere of instructed criticism most friendly to the growth of truth.

Elsewhere I have noted the paradox that the appearance of the work of Darwin, which crowns the great period in the study of the phenomena of species, was the signal for a general halt. The 'Origin of Species,' the treatise which for the first time brought the problem of species fairly within the range of human intelligence, so influenced the course of scientific thought that the study of this particular phenomenon—specific difference—almost entirely ceased. That this was largely due to the simultaneous opening up of lines of research in many other directions may be granted; but in greater measure, I believe, it is to be ascribed to the substitution of a conception of species which, with all the elements of truth it contains, is yet barren and unnatural. It is not wonderful that those who held that specific difference must be a phenomenon of slowest accumulation, proceeding by steps needing generations for their perception, should turn their attention to subjects deemed more amenable to human enterprise.

The indiscriminate confounding of all divergences from type into one heterogeneous heap under the name 'Variation' effectually concealed those features of order which the phenomena severally present, creating an enduring obstacle to the progress of evolutionary science. Specific normality and distinctness being regarded as an accidental product of exigency, it was thought safe to treat departures from such normality as comparable differences: all were 'variations' alike.

Let us illustrate the consequences. Princess of Wales is a large modern violet, single, with stalks a foot long or more. Marie Louise is another, with large double flowers, pale colour, short stalks, peculiar scent, leaf, &c. We call these 'varieties,' and we speak of the various fixed differences between these two, and between them and wild *odorata*, as due to variation; and, again, the transient differences between the same *odorata* in poor, dry soil, or in a rich hedge-bank, we call variation, using but the one term for differences, quantitative or qualitative, permanent or transitory, in size, number of parts, chemistry, and the rest. We might as well use one term to denote the differences between a bar of silver, a stick of lunar caustic, a shilling, or a teaspoon. No wonder that the ignorant tell us they can find no order in variation.

This prodigious confusion, which has spread obscurity over every part of these inquiries, is traceable to the original misconception of the nature of specific difference, as a thing imposed and not inherent. From this, at least, the earlier experimenters were free; and the undertakings of Gärtner and his contemporaries were informed by the true conception that the properties and behaviour of species were themselves specific. Free from the later fancy that but for selection the forms of animals and plants would be continuous and indeterminate, they recognised the definiteness of species and variety, and boldly set themselves to work out case by case the manifestations and consequences of that definiteness.

Over this work of minute and largely experimental analysis, rapidly growing, the new doctrine that organisms are mere conglomerates of adaptative devices descended like a numbing spell. By an easy confusion of thought, faith in the physiological definiteness of species and variety passed under the common ban which had at last exorcised the demon Immutability. Henceforth no naturalist must hold communion with either, on pain of condemnation as an apostate, a danger to the dynasty of Selection. From this oppression we in England, at least, are scarcely beginning to emerge. Bentham's 'Flora,' teaching very positively that the primrose, the cowslip, and the oxlip are impermanent varieties of one species, is in the hand of every beginner, while the British Museum Reading Room finds it unnecessary to procure Gärtner's '*Bastarderzeugung*.'

And so this mass of specific learning has passed out of account. The evidence of the collector, the horticulturist, the breeder, the fancier, has been treated with neglect, and sometimes, I fear, with contempt. That wide field whence Darwin drew his wonderful store of facts has been some forty years untouched. Speak to professional zoologists of any breeder's matter, and how many will not intimate to you politely that fanciers are unscientific persons, and their concerns beneath notice? For the concrete in evolution we are offered the abstract. Our philosophers debate with great fluency whether between imaginary races sterility could grow up by an imaginary Selection; whether Selection working upon hypothetical materials could produce sexual differentiation; how under a system of Natural Selection bodily symmetry may have been impressed on formless protoplasm—that monstrous figment of the mind, fit starting-point for such discussions. But by a physiological irony enthusiasm for these topics is sometimes fully correlated with indifference even to the classical illustrations; and for many whose minds are attracted by the abstract problem of inter-racial sterility there are few who can name for certain ten cases in which it has been already observed.

And yet in the natural world, in the collecting-box, the seed-bed, the poultry-yard, the places where variation, heredity, selection may be seen in operation and their properties tested, answers to these questions meet us at every turn—fragmentary answers, it is true, but each direct to the point. For if any one will stoop to examine Nature in those humble places, will do a few days' weeding, prick out some rows of cabbages, feed up a few score of any variable larva, he will not wait long before he learns the truth about variation. If he go further and breed two or three generations of almost any controllable form, he will obtain immediately facts as to the course of heredity which obviate the need for much laborious imagining. If strictly trained, with faith in the omnipotence of selection, he will not proceed far before he encounters disquieting facts. Upon

whatever character the attention be fixed, whether size, number, form of the whole or of the parts, proportion, distribution of differentiation, sexual characters, fertility, precocity or lateness, colour, susceptibility to cold or to disease—in short, all the kinds of characters which we think of as best exemplifying specific difference, we are certain to find illustrations of the occurrence of departures from normality, presenting exactly the same definiteness elsewhere characteristic of normality itself. Again and again the circumstances of their occurrence render it impossible to suppose that these striking differences are the product of continued selection, or, indeed, that they represent the results of a gradual transformation of any kind. Whenever by any collocation of favouring circumstances such definite novelties possess a superior viability, supplanting their 'normal' relatives, it is obvious that new types will be created.

The earliest statement of this simple inference is, I believe, that of Marchant,[1] who in 1719, commenting on certain plants of *Mercurialis* with laciniated and hair-like leaves, which for a time established themselves in his garden, suggested that species may arise in like manner. Though the same conclusion has appeared inevitable to many, including authorities of very diverse experience, such as Huxley, Virchow, F. Galton, it has been strenuously resisted by the bulk of scientific opinion, especially in England. Lately, however, the belief in Mutation, as De Vries has taught us to call it, has made notable progress,[2] owing to the publication of his splendid collection of observations and experiments, which must surely carry conviction of the reality and abundance of Mutation to the minds of all whose judgments can be affected by evidence.

That the dread test of Natural Selection must be passed by every aspirant to existence, however brief, is a truism which needs no special proof. Those who find satisfaction in demonstrations of the obvious may amply indulge themselves by starting various sorts of some annual, say French poppy, in a garden, letting them run to seed, and noticing in a few years how many of the finer sorts are represented; or by sowing an equal number of seeds taken from several varieties of carnation, lettuce, or auricula, and seeing in what proportions the fine kinds survive in competition with the common.

Selection is a true phenomenon; but its function is to *select*, not to create. Many a white-edged poppy may have germinated and perished before Mr. Wilks saved the individual which in a few generations gave rise to the Shirleys. Many a black *Amphidasys betularia* may have emerged before, some sixty years ago, in the urban conditions of Manchester the black var. *doubledayaria* found its chance, soon practically superseding the type in its place of origin, extending itself over England, and reappearing even in Belgium and Germany.

Darwin gave us sound teaching when he compared man's selective operations with those of Nature. Yet how many who are ready to expound Nature's methods have been at the pains to see how man really proceeds? To the domesticated form our fashions are what environmental exigency is to the wild. For years the conventional Chinese primrose threw sporadic plants of the loose-growing *stellata* variety, promptly extirpated because repugnant to mid-Victorian primness. But when taste, as we say, revived, the graceful Star Primula was saved by Messrs. Sutton, and a stock raised which is now of the highest fashion. I dare assert that few botanists meeting *P. stellata* in Nature would hesitate to declare it a good species. This and the Shirleys precisely illustrate the procedure of the raiser of novelties. His operations start from a definite beginning. As in the case of *P. stellata*, he may notice a mutational form thrown off perfect from the start, or, as in the Shirleys, what catches his attention may be the first indication of that

[1] Marchant, *Mém. Ac. roy. des sci.* for 1719; 1721, p. 59, Pls. 6–7. I owe this reference to Coutagne, *L'hérédité chez les vers à soie* (*Bull. sci. Fr. Belg.*, 1902).

[2] This progress threatens to be rapid indeed. Since these lines were written Professor Hubrecht, in an admirable exposition (*Pop. Sci. Monthly*, July 1904) of De Vries' *Mutations-theorie*, has even blamed me for having ten years ago attached *any* importance to continuous variation. Nevertheless, when the unit of segregation is small, something mistakably like continuous evolution must surely exist. (Cp. Johannsen, *Ueb. Erblichkeit in Populationen und in reinen Linien*, 1903.)

flaw which if allowed to extend will split the type into a host of new varieties each with its own peculiarities and physiological constitution.

Let anyone who doubts this try what he can do by selection without such a definite beginning. Let him try from a pure strain of black and white rats to raise a white one by breeding from the whitest, or a black one by choosing the blackest. Let him try to raise a dwarf ('Cupid') sweet pea from a tall race by choosing the shortest, or a crested fowl by choosing the birds with most feather on their heads. To formulate such suggestions is to expose their foolishness.

The creature is beheld to be very good after, not before its creation. Our domesticated races are sometimes represented as so many incarnations of the breeder's prophetic fancy. But except in recombinations of pre-existing characters—now a comprehensible process—and in such intensifications and such finishing touches as involve variations which analogy makes probable, the part played by prophecy is small. Variation leads; the breeder follows. The breeder's method is to notice a desirable novelty, and to work up a stock of it, picking up other novelties in his course—for these genetic disturbances often spread—and we may rest assured the method of Nature is not very different.

The popular belief that evolution, whether natural or artificial, is effected by mass-selection of impalpable differences arises from many errors which are all phases of one—imperfect analysis—though the source of the error differs with the circumstances of its exponent. When the scientific advocate professes that he has statistical proofs of the continuity of variation, he is usually availing himself of that comprehensive use of the term Variation to which I have referred. Statistical indications of such continuity are commonly derived from the study, not of nascent varieties, but of the fluctuations to which all normal populations are subject. Truly varying material needs care in its collection, and if found is often sporadic or in some other way unsuitable for statistical treatment. Sometimes it happens that the two phenomena are studied together in inextricable entanglement, and the resulting impression is a blur.

But when a practical man, describing his own experience, declares that the creation of his new breed has been a very long affair, the scientist, feeling that he has found a favourable witness, puts forward this testimony as conclusive. But on cross-examination it appears that the immense period deposed to seldom goes back beyond the time of the witness's grandfather, covering, say, seventy years; more often ten, or eight, or even five years will be found to have accomplished most of the business. Next, in this period—which, if we take it at seventy years, is a mere point of time compared with the epochs of which the selectionist discourses—a momentous transformation has often been effected, not in one character but many. Good characters have been added, it may be, of form, fertility, precocity, colour, and other physiological attributes, undesirable qualities have been eliminated, and all sorts of defects 'rogued' out. On analysis these operations can be proved to depend on a dozen discontinuities. Be it, moreover, remembered that within this period, besides *producing* his mutational character and combining it with other characters (or it may be groups of characters), the breeder has been working up a *stock*, reproducing in quantity that quality which first caught his attention, thus converting, if you will, a phenomenon of individuals into a phenomenon of a mass, to the future mystification of the careless.

Operating among such phenomena the gross statistical method is a misleading instrument; and, applied to these intricate discriminations, the imposing Correlation Table into which the biometrical Procrustes fits his arrays of unanalysed data is still no substitute for the common sieve of a trained judgment. For nothing but minute analysis of the facts by an observer thoroughly conversant with the particular plant or animal, its habits and properties, checked by the test of crucial experiment, can disentangle the truth.

To prove the reality of Selection as a factor in evolution is, as I have said, a work of supererogation. With more profit may experiments be employed in defining the *limits* of what Selection can accomplish. For whenever we can advance no further by Selection, we strike that hard outline fixed by the natural

properties of organisms. We come upon these limits in various unexpected places, and to the naturalist ignorant of breeding nothing can be more surprising or instructive.

Whatever be the mode of origin of new types, no theoretical evolutionist doubts that Selection will enable him to fix his character when obtained. Let him put his faith into practice. Let him set about breeding canaries to win in the class for Clear Yellow Norwich at the Crystal Palace Show. Being a selectionist, his plan will be to pick up winning yellow cocks and hens at shows and breed them together. The results will be disappointing. Not getting what he wants, he may buy still better clear yellows and work them in, and so on till his funds are exhausted, but he will pretty certainly breed no winner, be he never so skilful. For no selection of winning yellows will make them into a breed. They must be formed afresh by various combinations of colours appropriately crossed and worked up. Though breeders differ as to the system of combinations to be followed, all would agree that selection of birds representing the winning type was a sure way to fail. The same is true for nearly all canary colours except in Lizards, and, I believe, for some pigeon and poultry colours also.

Let this scientific fancier now go to the Palace Poultry Show and buy the winning Brown Leghorn cock and hen, breed from them, and send up the result of such a mating year after year. His chance of a winner is not quite, but almost *nil*. For in its wisdom the fancy has chosen one type for the cock and another for the hen. They belong to distinct strains. The hen corresponding to the winning cock is too bright, and the cock corresponding to the winning hen is too dull for the judge's taste. The same is the case in nearly every breed where the sex-colours differ markedly. Rarely winners of both sexes have come in one strain—a phenomenon I cannot now discuss—but the contrary is the rule. Does anyone suppose that this system of 'double mating' would be followed, with all the cost and trouble it involves, if Selection could compress the two strains into one? Yet current theory makes demands on Selection to which this is nothing.

The tyro has confidence in the power of Selection to fix type, but he never stops to consider what fixation precisely means. Yet a simple experiment will tell him. He may go to a great show and claim the best pair of Andalusian fowls for any number of guineas. When he breeds from them he finds, to his disgust, that only about half their chickens, or slightly more, come blue at all, the rest being blacks or splashed whites. Indignantly, perhaps, he will complain to the vendor that he has been supplied with no selected breed, but worthless mongrels. In reply he may learn that beyond a doubt his birds come from blues only in the direct line for an indefinite number of generations, and that to throw blacks and splashed whites is the inalienable property of blue Andalusians. But now let him breed from his 'wasters,' and he will find that the extracted blacks are *pure* and give blacks only, that the splashed whites similarly give only whites or splashed whites—but if the two sorts of 'wasters' are crossed together *blues only* will result. *Selection* will never make the blues breed true; nor can this ever come to pass unless a blue be found whose germ-cells are bearers of the blue character—which may or may not be possible. If the selectionist reflect on this experience he will be led straight to the centre of our problem. There will fall, as it were, scales from his eyes, and in a flash he will see the true meaning of fixation of type, variability, and mutation, vaporous mysteries no more.

Owing to the unhappy subdivisions of our studies, such phenomena as these—constant companions of the breeder—come seldom within the purview of modern science, which, forced for a moment to contemplate them, expresses astonishment and relapses into indolent scepticism. It is in the hope that a little may be done to draw research back into these forgotten paths that I avail myself of this great opportunity of speaking to my colleagues with somewhat wider range of topic than is possible within the limits of a scientific paper. For I am convinced that the investigation of heredity by experimental methods offers the sole chance of progress with the fundamental problems of evolution.

In saying this I mean no disrespect to that study of the physiology of reproduction by histological means, which, largely through the stimulus of Weismann's speculations, has of late made such extraordinary advances. It needs no penetration to see that, by an exact knowledge of the processes of maturation and fertilisation, a vigorous stock is being reared, upon which some day the experience of the breeder will be firmly grafted, to our mutual profit. We, who are engaged in experimental breeding, are watching with keenest interest the researches of Strasburger, Boveri, Wilson, Farmer, and their many fellow-workers and associates in this difficult field, sure that in the near future we shall be operating in common. We know already that the experience of the breeder is in no way opposed to the facts of the histologist; but the point at which we shall unite will be found when it is possible to trace in the maturing germ an indication of some character afterwards recognisable in the resulting organism. Till then, in order to pursue directly the course of heredity and variation, it is evident that we must fall back on those tangible manifestations which are to be studied only by field observation and experimental breeding.

The breeding-pen is to us what the test-tube is to the chemist—an instrument whereby we examine the nature of our organisms and determine empirically what for brevity I may call their genetic properties. As unorganised substances have their definite properties, so have the several species and varieties which form the materials of our experiments. Every attempt to determine these definite properties contributes immediately to the solution of that problem of problems, the physical constitution of a living organism. In those morphological studies which I suppose most of us have in our time pursued, we sought inspiration from the belief that in the examination of present normalities we were tracing the past, the phylogenetic order of our types, the history—as we conceived—of Evolution. In the work which I am now pressing upon your notice we may claim to be dealing not only with the present and the past, but with the future also.

On such an occasion as this it is impossible to present to you in detail the experiments—some exceedingly complex—already made in response to this newer inspiration. I must speak of results, not of methods. At a later meeting, moreover, there will be opportunities of exhibiting practically to those interested some of the more palpable illustrations. It is also impossible to-day to make use of the symbolic demonstrations by which the lines of analysis must be represented. The time cannot be far distant when ordinary Mendelian formulæ will be mere *as in praesenti* to a biological audience. Nearly five years have passed since this extraordinary re-discovery was made known to the scientific world by the practically simultaneous papers of De Vries, Correns, and Tschermak, not to speak of thirty-five years of neglect endured before. Yet a phenomenon comparable in significance with any that biological science has revealed remains the intellectual possession of specialists. We still speak sometimes of Mendel's hypothesis or theory, but in truth the terms have no strict application. It is no theory that water is made up of hydrogen and oxygen, though we cannot watch the atoms unite, and it is no theory that the blue Andalusian fowl I produce was made by the meeting of germ-cells bearing respectively black and a peculiar white. Both are incontrovertible facts deduced from observation. The two facts have this in common also, that their perception gives us a glimpse into that hidden order out of which the seeming disorder of our world is built. If I refer to Mendelian 'theory' therefore, in the words with which Bacon introduced his Great Instauration, 'I entreat men to believe that it is not an opinion to be held, but a work to be done; and to be well assured that I am labouring to lay the foundation, not of any sect or doctrine, but of human utility and power.'

In the Mendelian method of experiment the one essential is that the posterity of each *individual* should be traced separately. If individuals from necessity are treated collectively, it must be proved that their composition is identical. In direct contradiction to the methods of current statistics, Mendel saw by sure penetration that masses must be avoided. Obvious as this necessity seems when one is told, no previous observer had thought of it, whereby the discovery was

missed. As Mendel immediately proved in the case of peas, and as we have now seen in many other plants and animals, it is often impossible to distinguish by inspection individuals whose genetic properties are totally distinct. Breeding gives the only test.

Segregation.

Where the proper precautions have been taken, the following phenomena have been proved to occur in a great range of cases, affecting many characters in some thirty plants and animals. The qualities or characters whose transmission in heredity is examined are found to be distributed among the germ-cells, or gametes, as they are called, according to a definite system. This system is such that these characters are treated by the cell-divisions (from which the gametes result) as existing in pairs, each member of a pair being alternative or *allelomorphic* to the other in the composition of the germ. Now, as every zygote—that is, any ordinary animal or plant—is formed by the union of two gametes, it may either be made by the union of two gametes bearing similar members of any pair, say two blacks or two whites, in which case we call it *homozygous* in respect of that pair, or the gametes from which it originates may be bearers of the dissimilar characters, say a black and a white, when we call the resulting zygote *heterozygous* in respect of that pair. If the zygote is homozygous, no matter what its parents or their pedigree may have been, it breeds true indefinitely unless some fresh variation occurs.

If, however, the zygote be heterozygous, or gametically cross-bred, its gametes in their formation separate the allelomorphs again, so that each gamete contains only one allelomorphic character of each pair. At least one cell-division in the process of gametogenesis is therefore a differentiating or *segregating* division, out of which each gamete comes sensibly pure in respect of the allelomorph it carries, exactly as if it had not been formed by a heterozygous body at all. That, translated into modern language, is the essential discovery that Mendel made. It has now been repeated and verified for numerous characters of numerous species, and, in face of heroic efforts to shake the evidence or to explain it away, the discovery of gametic segregation is, and will remain, one of the lasting triumphs of the human mind.

In extending our acquaintance of these phenomena of segregation we encounter several principal types of complication.

Segregation Absent or Incomplete.—From our general knowledge of breeding we feel fairly well satisfied that true absence of segregation is the rule in certain cases. It is difficult, for instance, to imagine any other account of the facts respecting the American Mulattos, though even here sporadic occurrence of segregation seems to be authenticated. Very few instances of genuine absence of segregation have been critically studied. The only one I can cite from my own experience is that of *Pararge egeria* and *egeriades*, 'climatic' races of a butterfly. When crossed together, they give the common intermediate type of North-Western France, which, though artificially formed, breeds in great measure true. This crossed back with either type has given, as a rule, simple blends between intermediate and type. My evidence is not, however, complete enough to warrant a positive statement as to the total absence of segregation, for in the few families raised from pairs of artificial intermediates some dubious indications of segregation have been seen.

The rarity of true failure of segregation when pure strains are crossed may be judged by the fact that since the revival of interest in such work hardly any thoroughly satisfactory cases have been witnessed. The largest body of evidence on this subject is that provided by De Vries. These cases, however, present so many complexities that it is impossible to deal with them now. While so little is definitely known regarding non-segregating characters, it appears to me premature to attempt any generalisation as to what does or does not segregate.

Most of the cases of failure of segregation formerly alleged are evidently spurious, depending on the appearance of homozygotes in the second generation (F_2).

One very important group of cases exists, in which the appearance of a *partial* failure of segregation after the second generation (F_2) is really due to another phenomenon. The visible character of a zygote may, for instance, depend on the coexistence in it of two characters belonging to distinct allelomorphic pairs, each capable of being independently segregated from its fellow, and forming independent combinations. For the demonstration of this important fact we are especially indebted to Cuénot.[1] We have indications of the existence of such a phenomenon in a considerable range of instances (mice, rabbits (Hurst), probably stocks and sweet peas).

Nevertheless, there are other cases, not always easy to distinguish from these, where *some* of the gametes of F_1 certainly carry on heterozygous characters unsegregated. As an example, which seems to me indisputable, I may mention the so-called 'walnut' comb, normal to Malay fowls. This can be made artificially by crossing rose-comb with pea-comb, and the crossbred then forms gametes, of which one in four bears the compound unsegregated.[2] We may speak of this as a true *synthesis*.

In another type of cases segregation occurs, but is not sharp. The gametes may then represent a full series ranging from the one pure form to the other. Such cases occur in regard to some colours of *Primula sinensis*, and the leg-feathering of fowls (Hurst). In the second generation a nearly complete series of intermediate zygotes may result, though the two pure extremes (if the case be one of blending characters) may still be found to be pure.

Resolution and Disintegration.—Besides these cases, the features of which we now in great measure comprehend, we encounter frequently a more complex segregation, imperfectly understood, by which gametes of new types, sometimes very numerous, are produced by the crossbred. Each of these new types has its own peculiarities. We shall, I think, be compelled to regard these phenomena as produced either by a *resolution* of compound characters introduced by one or both parents, or by some process of *disintegration*, effected by a breaking-up of the integral characters followed by recombinations. It seems impossible to imagine simple recombinations of pre-existing characters as adequate to produce many of these phenomena. Such a view would involve the supposition that the number of characters pre-existing as units was practically infinite—a difficulty that as yet we are not obliged to face. However that may be, we have the fact that resolutions and disintegrations of this kind—or recombinations, if that conception be preferred—are among the common phenomena following crossing, and are the sources of most of the breeder's novelties. As bearing on the theoretical question to which I have alluded, we may notice that it is among examples of this complex breaking-up that a great proportion of the cases of partial sterility have been seen.

No quite satisfactory proof as to the actual moment of segregation yet exists, nor have we any evidence that all characters are segregated at the same cell-division. Correns has shown that in maize the segregation of the starch character from the sugar character must happen before the division forming the two generative nuclei, for both bear the same character. The reduction-division has naturally been suggested as the critical moment. The most serious difficulty in accepting this

[1] When $abc \ldots \times \alpha\beta\gamma \ldots$ gives in F_1 or F_2 a character (not seen in the original parents), which from F_2 or later may breed true: not because $a\alpha$, $b\beta$, $c\gamma$ do not severally segregate, but through simultaneous homozygosis of, say, aa and $\beta\beta$, giving a zygote $aa\beta\beta c\gamma \ldots$ which will breed true to the character $a\beta$.

[2] Owing to this behaviour, and to the simultaneous production of single-comb (? by resolution), there are, even in pure Malays, five types of individuals, all with 'walnut' combs—as yet indistinguishable—formed by gametic unions $r \times p$, $rp \times rp$, $rp \times r$, $rp \times p$, $rp \times s$. Of these kinds three can at once be distinguished by crossing with single; but whether $r \times p$ can be distinguished from $rp \times s$ we do not yet know. [*r*, rose; *p*, pea; *s*, single; *rp*, walnut.] In this example four allelomorphs are simultaneously segregated, one being compound. Neglecting sexual differentiation, there are therefore *ten* gametically distinct types theoretically possible; but of these only *four* are distinguishable by inspection.

view, as it seems to me, is the fact that somatic divisions appear sometimes to segregate allelomorphs, as in the case of Datura fruits, and some colour-cases.

In concluding this brief notice of the complexities of segregation I may call attention to the fact that we are here engaged in no idle speculation. For it is now possible by experimental means to distinguish almost always with which phenomenon we are dealing, and each kind of complication may be separately dealt with by a determination of the properties of the extracted forms. Illustrations of a practical kind will be placed before you at a subsequent meeting.

The consequence of segregation is that in cases where it occurs we are rid of the interminable difficulties which beset all previous attempts to unravel heredity. On the older view, the individuals of any group were supposed to belong to an indefinite number of classes, according to the various numerical proportions in which various types had entered into their pedigree. We now recognise that when segregation is allelomorphic, as it constantly is, the individuals are of three classes only in respect of each allelomorphic pair—two homozygous and one heterozygous. In all such cases, therefore, fixity of type, instead of increasing gradually generation by generation, comes suddenly, and is a phenomenon of individuals. Only by the separate analysis of individuals can this fact be proved. The supposition that progress towards fixity of type was gradual arose from the study of masses of individuals, and the gradual purification witnessed was due in the main to the gradual elimination of impure individuals, whose individual properties were wrongly regarded as distributed throughout the mass.

We have at last the means of demonstrating the presence of integral characters. In affirming the integrity of segregable characters we do not declare that the size of the integer is fixed eternally, as we suppose the size of a chemical unit to be. The integrity of our characters depends on the fact that they *can* be habitually treated as units by gametogenesis. But even where such unity is manifested in its most definite form, we may, by sufficient searching, generally find a case where the integrity of the character has evidently been impaired in gametogenesis, and where one such individual is found the disintegration can generally be propagated. That the size of the unit may be changed by unknown causes, though a fact of the highest significance in the attempt to determine the physical nature of heredity, does not in the least diminish the value of the recognition of such units, or lessen their part in governing the course of Evolution.

The existence of unit-characters had, indeed, long been scarcely doubtful to those practically familiar with the facts of variation,[1] but it is to the genius of Mendel that we owe the proof. We knew that characters could behave as units, but we did not know that this unity was a phenomenon of gametogenesis. He has revealed to us the underworld of gametes. Henceforth, whenever we see a preparation of germ-cells we shall remember that, though all may look alike, they may in reality be of many and definite kinds, differentiated from each other according to regular systems.

Numerical Relations of Gametes and their Significance.

In addition to the fact of segregation, Mendel's experiments proved another fact nearly as significant; namely, that when characters are allelomorphic, the gametes bearing each member of a pair generally are formed in equal numbers by the heterozygote, if an average of cases be taken. This fact can only be regarded as a consequence of some numerical symmetry in the cell-divisions of gametogenesis. We already know cases where individual families show such departure from normal expectation that either the numbers produced must have been unequal, or subsequent disturbance must have occurred. But so far no case is known *for certain* where the average of families does not point to equality.

The fact that equality is so usual has a direct bearing on conceptions of the physical nature of heredity. I have compared our segregation with chemical

[1] Cp. De Vries, *Intracellulare Pangenesis*, 1889.

separation, but the phenomenon of numerically symmetrical disjunction as a feature of so many and such different characters seems scarcely favourable to any close analogy with chemical processes. If each special character owed its appearance to the handing on of some complex molecule as a part of one chemical system, we should expect, among such a diversity of characters and forms of life, to encounter some phenomenon of valency, manifested as numerical inequality between members of allelomorphic pairs. So far, equivalence is certainly the rule, and where the characters are simply paired and no resolution has taken place, this rule appears to be universal as regards averages. On the other hand, there are features in the distribution of characters after resolution, when the second generation (F_2) is polymorphic in a high degree, which are not readily accounted for on any hypothesis of simple equivalence; but none of these cases are as yet satisfactorily investigated.

It is doubtful whether segregation is rightly represented as the separation of *two* characters, and whether we may not more simply imagine that the distinction between the allelomorphic gametes is one of presence or absence of some distinguishing element. De Vries has devoted much attention to this question in its bearings on his theory of Pangenesis, holding that cases of both kinds occur, and attempting to distinguish them. Indications may certainly be enumerated pointing in either direction, but for the present I incline to defer a definite opinion.

If we may profitably seek in the physical world for some parallel to our gametic segregations, we shall, I think, find it more close in mechanical separations, such as those which may be effected between fluids which do not freely mix, than in any strictly chemical phenomenon. In this way we might roughly imitate both the ordinary segregation, which is sensibly perfect, and the curious impurity occasionally perceptible even in the most pronounced discontinuities, such as those which divide male from female, petal from sepal, albino from coloured, horn from hair, and so on.

Gametic Unions and their Consequences.

Characters being then distributable among gametes according to regular systems, the next question concerns the properties and features presented by the zygotes formed by the union of gametes bearing different characters.

As to this no rule can as yet be formulated. Such a heterozygote may exhibit one of the allelomorphic characters in its full intensity (even exceeding it in special cases, perhaps in connection with increased vigour), or it may be intermediate between the two, or it may present some character not recognisable in either parent. In the latter case it is often, though not always, reversionary. When one character appears in such intensity as to conceal or exclude the other it is called *dominant*, the other being *recessive*. It may be remarked that frequently, but certainly not universally (as has been stated), the phylogenetically older character is dominant. A curious instance to the contrary is that of the peculiar arrangement of colours seen in a breed of game fowls called Brown breasted, which in combination with the purple face, though certainly a modern variation, dominates (most markedly in females) over the Black-breasted type of *Gallus bankiva*.

In a few cases irregularity of dominance has been observed as an exception. The clearest illustration I can offer is that of the extra toe in fowls. Generally this is a dominant character, but sometimes, as an exceptional phenomenon, it may be recessive, making subsequent analysis very difficult. The nature of this irregularity is unknown. A remarkable instance is that of the blue colour in maize seeds (Correns; R. H. Lock). Here the dominance of blue is frequently imperfect, or absent, and the figures suggest that some regularity in the phenomenon may be discovered.

Mendel is often represented as having enunciated dominance as a general proposition. That this statement should still be repeated, even by those who realise the importance of his discoveries, is an extraordinary illustration of the oblivion that has overwhelmed the work of the experimental breeders. Mendel makes the specific statement in regard to certain characters in peas which do behave thus, but his proposition is not general. To convict him of such a delusion

it would be necessary to prove that he was exceptionally ignorant of breeding, though on the face of the evidence he seems sufficiently expert.

A generalisation respecting the consequences of heterozygosis possessing greater value is this. When a pair of gametes unites in fertilisation the characters of the zygote depend directly on the constitution of these gametes, and not on that of the parents from which they came. To this generalisation we know as yet only two clear exceptions. These very curious cases are exactly alike in that, though segregation obviously occurs in a seed-character, the seeds borne by the hybrid (F_1) all exhibit the hybrid character, and the consequences of segregation in the particular seed-character are not evident till the seeds (F_3) of the second (F_2) generation are determinable. Of these the first is the case of indent peas investigated especially by Tschermak. Crossed with wrinkled peas I have found the phenomena normal, but when the cross is made with a round type the exceptional phenomenon occurs. The second case is that discovered by Biffen in the cross between the long-grained wheat called Polish and short-grained Rivett wheat, demonstrations of which will be laid before you. No satisfactory account of these peculiarities has been yet suggested, but it is evident that in some unexplained way the maternal plant-characters control the seed-characters for each generation. It is, of course, likely that other comparable cases will be found

Appearances have been seen in at least four cases (rats, mice, stocks, sweet peas) suggesting at first sight that a heterozygosis between two gametes, *both* extracted, may give, *e.g.*, dominance; while if one, or both, were pure, they would give a reversionary heterozygote. If this occurrence is authenticated on a sufficient scale, we shall of course recognise that the fact proves the presence in these cases of some pervading and non-segregating quality, distributed among the extracted gametes formed by the parent heterozygote. As yet, however, I do not think the evidence enough to warrant the conclusion that such a pervading quality is really present, and I incline to attribute the appearances to redistribution of characters belonging to independent pairs in the manner elucidated by Cuénot. The point will be easily determined, and meanwhile we must note the two possibilities.

Following, therefore, our first proposition that the gametes belong to definite classes, comes the second proposition, that the unions of members of the various classes have specific consequences. Nor is this proposition simply the truistical statement that different causes have different effects; for by its aid we are led at once to the place where the different cause is to be sought—Gametogenesis. While formerly we hoped to determine the offspring by examining the *ancestry* of the parents, we now proceed by investigating the *gametic composition* of the parents. Individuals may have identical ancestry (and sometimes, to all appearances, identical characters), but yet be quite different in gametic composition; and, conversely, individuals may be identical in gametic composition and have very different ancestry. Nevertheless, those that are identical in gametic composition are the same, whatever their ancestry. Therefore, where such cases are concerned, in any considerations of the physiology of heredity, ancestry is misleading and passes out of account. To take the crudest illustration, if a hybrid is made between two races, A, B, and another hybrid between two other races, C, D, it might be thought that when the two hybrids AB and CD are bred together, four races, A, B, C, and D, will be united in their offspring. This expectation may be entirely falsified, for the cell-divisions of gametogenesis may have split A from B and C from D, so that the final product may contain characters of only two races after all, being either AC, BC, AD, or BD. In practice, however, we are generally dealing with *groups* of characters, and the union of all the A group, for instance, with all the C group will be a rare coincidence.

It is the object of Mendelian analysis to state each case of heredity in terms of gametic composition, and thence to determine the laws governing the distribution of characters in the cell-divisions of gametogenesis.

There are, of course, many cases which still baffle our attempts at such analysis, but some of the most paradoxical exceptions have been reduced to order by the accumulation of facts. The consequences of heterozygosis are curiously specific,

and each needs separate investigation. A remarkable case occurred in stocks, showing the need for caution in dealing with contradictory results. Hoary leaves and glabrous leaves are a pair of allelomorphic characters. When glabrous races were crossed with crossbreds, sometimes the results agreed with simple expectation, while in other cases the offspring were all hoary when, in accordance with similar expectation, this should be impossible. By further experiment, however, Miss Saunders has found that *certain* glabrous races crossed together give nothing but hoary heterozygotes, which completely elucidates such exceptions. There is every likelihood that wherever segregation occurs similar analysis will be successful.

Speaking generally, in every case the first point to be worked out is the magnitude of the character-units recognised by the critical cell-divisions of gametogenesis, and the second is the specific consequence of all the possible combinations between them. When this has been done for a comprehensive series of types and characters, it will be time to attempt further generalisation, and perhaps to look for light on that fundamental physiological property, the power of cell-division.

Segregation and Sex.—Acquaintance with Mendelian phenomena irresistibly suggests the question whether in *all* cases of families composed of distinct types the distinctness may not be primarily due to gametic segregation. Of all such distinctions none is so universal or so widespread as that of sex: may it not be possible that sex is due to a segregation occurring between gametes, either male, female, or both? It will be known to you that several naturalists have been led by various roads to incline to this view. We still await the proof of crucial experiments; but without taking you over more familiar ground, it may be useful to show how the matter looks from our standpoint. As regards actual experiment, all results thus far are complicated by the occurrence of some sterility in the hybrid generation. Correns, fertilising ♀ *Bryonia dioica* with pollen from ☿ *B. alba* obtained offspring (F_1) *either* ♂ or ♀, with only one doubtful exception. Gärtner found a similar result in *Lychnis diurna* ♀ × ☿ *L. Flos-cuculi* as ♂, but only raised six plants (4 ♂, 2 ♀). From *L. diurna* ♀ × ☿ *Silene noctiflora* as ♂ he got only two plants, spoken of as females which developed occasional anthers. These results give a distinct suggestion that sex may be determined by differentiation among the male gametes, but satisfactory and direct proofs can only be obtained from some case where sterility does not ensue.

Apart, however, from such decisive evidence—which, indeed, would be more satisfactory if relating to *animals*—several circumstances suggest that sex is a segregation-phenomenon. Professor Castle in a valuable essay has called attention to distinct evidence of disturbance in the heredity of certain moths (*Aglia tau* and *lugens*, Standfuss's experiments; *Tephrosia*, experiments of Bacot and others, summarised by Tutt),[1] where the disturbance is pretty certainly connected with sexual differentiation. Mr. Punnett and I are finding suggestions of the same thing in certain poultry cases. Mr. Doncaster has pointed out that the evidence of Mr. Raynor clearly indicates that a certain variety of *Abraxas grossulariata*, usually peculiar to the female, is a Mendelian recessive. It is scarcely doubtful that this will be shown to hold also for some other female varieties, *e.g.*, *Colias edusa*, var. *helice*, &c. We can therefore feel no doubt that there is some entanglement between sex and gametically segregable characters. A curious instance of a comparable nature is that of the Cinnamon canary (Norduijn, &c.), and similar complications are alleged as regards the descent of colour-blindness and hæmophilia.

In one remarkable group of facts we come very near to the phenomenon of sex. Experiments made in conjunction with Mr. R. P. Gregory have shown that the familiar heterostylism of *Primula* is a phenomenon of Mendelian segregation. Short style, or 'thrum,' is a dominant—with a complication;[2] long style, or 'pin,' is recessive; while equal, or 'homostyle,' is recessive to both.

[1] *Trans. Ent. Soc. Lond.*, 1898.

[2] It is doubtful if 'thrum' ever breeds true, as both the other types can do. Perhaps 'thrum' is a *Halbrasse* of De Vries.

Even nearer we come in a certain sweet-pea example, where abortion of anthers behaves as an ordinary Mendelian recessive character.[1] By a slight exaggeration we might even speak of a hermaphrodite with barren anthers as a 'female.'

Consider also how like the two kinds of differentiation are. The occasional mosaicism in Lepidoptera, called 'gynandromorphism,' may be exactly paralleled by specimens where the two halves are two colour-varieties, instead of the two sexes. Patches of *Silene inflata* in this neighbourhood commonly consist of hairy and glabrous individuals,[2] a phenomenon proved in *Lychnis* to be dependent on Mendelian segregation. The same patch consists also of female plants and hermaphrodite plants. Is it not likely that both phenomena are similar in nature? How otherwise would the differentiation be maintained? The sweet-pea case I have spoken of is scarcely distinguishable from this. I therefore look forward with confidence to the elucidation of the real nature of sex—that redoubtable mystery.

We now move among the facts with an altogether different bearing. 'Animals and Plants under Domestication,' from being largely a narration of inscrutable prodigies, begins to take shape as a body of coherent evidence. Of the old difficulties many disappear finally. Others are inverted. Darwin says he would have expected 'from the law of reversion' that nectarines being the newer form would more often produce peaches than peaches nectarines, which is the commoner occurrence. Now, on the contrary, the unique instance of the Carclew nectarine tree bearing peaches is more astonishing than all the other evidence together!

Though the progress which Mendelian facts make possible is so great, it must never be forgotten that as regards new characters involving the addition of some new factor to the pre-existing stock we are almost where we were. When they have been added by mutation, we can now study their transmission; but we know not whence or why they come. Nor have we any definite light on the problem of adaptation; though here there is at least no increase of difficulties.

Besides these outstanding problems, there remain many special points of difficulty which on this occasion I cannot treat—curiosities of segregation, obscure aberrations of fertilisation[3] (occasionally met with), coupling of characters, and the very serious possibility of disturbance through gametic selection. Let us employ the space that remains in returning to the problem of variation, already spoken of above, and considering how it looks in the light of the new facts as to heredity. The problem of heredity is the problem of the manner of distribution of characters among germ-cells. So soon as this problem is truly formulated, the nature of variation at once appears. For the first time in the history of evolutionary thought, Mendel's discovery enables us to form some picture of the process which results in genetic variation. It is simply the segregation of a new kind of gamete, bearing one or more characters distinct from those of the type. We can answer one of the oldest questions in philosophy. In terms of the ancient riddle, we may

[1] Neglecting minor complications, the descent is as follows:—Lady Penzance ♀ × Emily Henderson (long pollen) ♂ gave purple F_1. In one F_2 family, with rare exceptions, *coloured* plants with *dark* axils were fertile, those with *light* axils having ♂ sterile, whites being either fertile or sterile. The ratios indicated are 9 coloured, dk. ax., fertile ♂ : 3 coloured, lt. ax., sterile ♂ : 3 white, fertile ♂ : 1 white, sterile ♂. The fertile whites, therefore, though light-axilled (as whites almost always are) presumably bear the dark-axil character, which generally cannot appear except in association with coloured flowers. This can be proved next year. Some at least of the plants with sterile ♂ are fertile on the ♀ side, and when crossed with a coloured light-axilled type will presumably give only light-axilled plants.

[2] This excellent illustration was shown me by Mr. A. W. Hill and Mr. A. Wallis. A third form, glabrous, with hairy edges to the leaves, also occurs.

[3] In view of Ostenfeld's discovery of parthenogenesis in *Hieracium*, the possibility that this phenomenon plays a part in some non-segregating cases needs careful examination.

reply that the Owl's egg existed before the Owl; and if we hesitate about the Owl, we may be sure about the Bantam. The parent zygote, whose offspring display variation, is giving off new gametes, and in its gametogenesis a segregation of their new character, more or less pure, is taking place. The significance and origin of the discontinuity of variation is therefore in great measure evident. So far as pre-existing elements are concerned, it is an expression of the power of cell-division to distribute character-units among gametes. The initial purity of so many nascent mutations is thus no longer surprising, and, indeed, that such initial purity has not been more generally observed we may safely ascribe to imperfections of method.

It is evident that the resemblance between the parent originating a variety and a heterozygote is close, and the cases need the utmost care in discrimination. If, for instance, we knew nothing more of the Andalusian fowl than that it throws blacks, blues, and whites, how should we decide whether the case was one of heterozygosis or of nascent mutation? The second (F_2) generation from Brown Leghorn × White Leghorn contains an occasional Silver-Grey or Duckwing female. Is this a *mutation* induced by crossing, or is it simply due to a recombination of pre-existing characters? We cannot yet point to a criterion which will certainly separate the one from the other; but perhaps the statistical irregularity usually accompanying mutation, contrasted with the numerical symmetry of the gametes after normal heterozygosis, may give indications in simple cases—though scarcely reliable even there. These difficulties reach their maximum in the case of types which are *continually* giving off a second form with greater or less frequency as a concomitant of their ordinary existence. This extraordinarily interesting phenomenon, pointed out first by De Vries, and described by him under the head of '*Halb-*' and '*Mittel-Rassen*,' is too imperfectly understood for me to do more than refer to it, but in the attempt to discover what is actually taking place in variation it must play a considerable part.

Just as that normal truth to type, which we call heredity, is in its simplest elements only an expression of that qualitative symmetry characteristic of all non-differentiating cell-divisions, so is genetic variation the expression of a qualitative asymmetry beginning in gametogenesis. Variation is a novel cell-division.[1] So soon as this fact is grasped we shall hear no more of heredity and variation as opposing 'factors' or 'forces'—a metaphor which has too long plagued us.

We cease, then, to wonder at the suddenness with which striking variations arise. Those familiar with the older literature relating to domesticated animals and plants will recall abundant instances of the great varieties appearing early in the history of a race, while the finer shades had long to be waited for. In the sweet pea the old purple, the red bicolor, and the white have existed for generations, appearing soon after the cultivation of the species; but the finer splitting which gave us the blues, pinks, &c., is a much rarer event, and for the most part only came when crossing was systematically undertaken. If any of these had been seen before by horticulturists, we can feel no doubt whatever they would have been saved. An observer contemplating a full collection of modern sweet peas, and ignorant of their history, might suppose that the extreme types had resulted from selective and more or less continuous intensification of these intermediates, exactly inverting the truth.

We shall recognise among the character-groups lines of cleavage, along which they easily divide, and other finer subdivisions harder to effect. Rightly considered, the sudden appearance of a total albino or a bicolor should surprise us less than the fact that the finer shades can appear at all.

At this point comes the inevitable question, what *makes* the character-group split? Crossing, we know, may do this; but if there be no crossing, what is the *cause* of variation? With this question we come sharply on the edge of human

[1] The parallel between the differentiating divisions by which the parts of the normal body are segregated from each other, and the segregating processes of gametogenesis, must be very close. Occasionally we even see the segregation of Mendelian characters among zygotic cells.

knowledge. But certain it is that if causes of variation are to be found by penetration, they must be specific causes. A mad dog is not 'caused' by July heat, nor a moss rose by progressive culture. We await our Pasteur; founding our hope of progress on the aphorism of Virchow, that every variation from type is due to a pathological accident, the true corollary of '*Omnis cellula e cellula*.'

In imperfect fashion I have now sketched the lines by which the investigation of heredity is proceeding, and some of the definite results achieved. We are asked sometimes, Is this new knowledge any use? That is a question with which we, here, have fortunately no direct concern. Our business in life is to find things out, and we do not look beyond. But as regards heredity, the answer to this question of use is so plain that we may give it without turning from the way.

We may truly say, for example, that even our present knowledge of heredity, limited as it is, will be found of extraordinary use. Though only a beginning has been made, the powers of the breeder of plants and animals are vastly increased. Breeding is the greatest industry to which science has never yet been applied. This strange anomaly is over; and, so far at least as fixation or purification of types is concerned, the breeder of plants and animals may henceforth guide his operations with a great measure of certainty.

There are others who look to the science of heredity with a loftier aspiration; who ask, Can any of this be used to help those who come after to be better than we are—healthier, wiser, or more worthy? The answer depends on the meaning of the question. On the one hand it is certain that a competent breeder, endowed with full powers, by the aid even of our present knowledge, could in a few generations breed out several of the morbid diatheses. As we have got rid of rabies and pleuro-pneumonia so we could exterminate the simpler vices. Voltaire's cry '*Écraser l'infâme*' might well replace Archbishop Parker's Table of Forbidden Degrees, which is all the instruction Parliament has so far provided. Similarly, a race may conceivably be bred true to some physical and intellectual characters considered good. The positive side of the problem is less hopeful, but the various species of mankind offer ample material. In this sense science already suggests the way. No one, however, proposes to take it; and so long as, in our actual laws of breeding, superstition remains the guide of nations, rising ever fresh and unhurt from the assaults of knowledge, there is nothing to hope or to fear from these sciences.

But if, as is usual, the philanthropist is seeking for some external application by which to ameliorate the course of descent, knowledge of heredity cannot help him. The answer to his question is *No*, almost without qualification. We have no experience of any means by which transmission may be made to deviate from its course; nor from the moment of fertilisation can teaching, or hygiene, or exhortation pick out the particles of evil in that zygote, or put in one particle of good. From seeds in the same pod may come sweet peas climbing five feet high, while their own brothers lie prone upon the ground. The stick will not make the dwarf peas climb, though without it the tall can never rise. Education, sanitation, and the rest, are but the giving or withholding of opportunity. Though in the matter of heredity every other conclusion has been questioned, I rejoice that in this we are all agreed.

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WITH THE BRITISH ASSOCIATION IN SOUTH AFRICA

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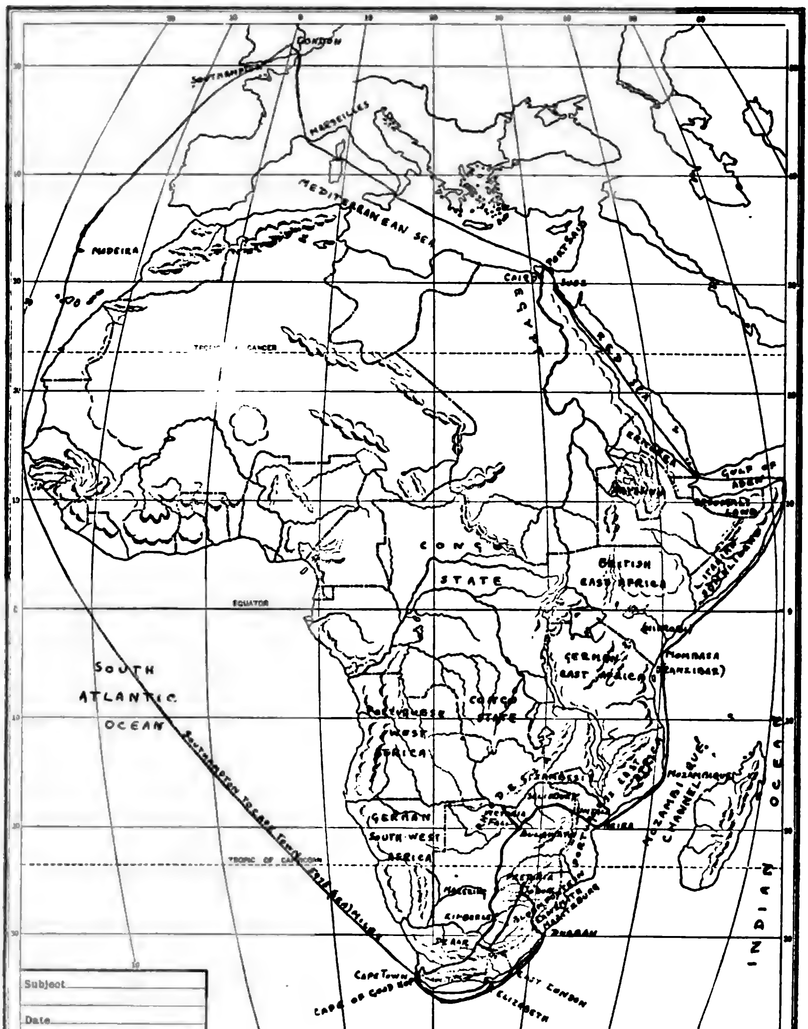
I.

THE visit of the British Association to South Africa during the past summer appears to have established the idea that its activities in future are not to be confined to the British Isles. Two successful oversea meetings had already taken place; the first at Montreal, in 1884, and the second at Toronto, in 1897, and there seemed to be no reason why the suggestion of a meeting in Cape Town, made as far back as 1898, by Sir David Gill, astronomer royal at the Cape, should not be followed up. But there were many difficulties in the way. It was obvious at the outset that few would be willing to make two long journeys by sea unless opportunities were afforded to visit the chief places of interest in other parts of South Africa. It was obvious too that few of those whose presence was chiefly desired would be in a position to afford the necessary expense unless very considerable assistance were forthcoming, and the general funds of the association were not intended, nor were they sufficient, for this purpose. Further, there are few towns where accommodation for several hundred visitors can be obtained, and this meant that special trains with dining and sleeping cars must be provided; the trunk lines in the colonies have a supply of rolling stock not much more than is sufficient for the few who travel long distances in South Africa.

While the matter was under discussion, war broke out. But those) were interested did not lose sight of the idea, and early last year ook more definite shape in generous offers of assistance from the ernments and towns in South Africa. In the meantime, many nges had occurred. The new colonies must be included in the ary; opportunities must be afforded to see places and districts

rendered famous during the war; the extension of the main line in Rhodesia to the Victoria Falls made a visit to this natural wonder almost a necessity; and the recent connection of the port of Beira in



SCALE. ABOUT 750 MILES TO THE INCH.
The main routes are marked with a thick line ——
Alternative routes by dotted lines ....
The dot and dash lines —·—· show political boundaries.
This map of Africa is copyrighted by the McKinley Publishing Company, Philadelphia, and is printed here by their courtesy.

Portuguese territory with Bulawayo suggested a possible return by the east coast and through the Suez Canal.

The tour finally planned was an extensive one, as a glance at the

accompanying map will show. The Union-Castle line steamers *Kildonan Castle* and *Durham Castle,* leaving Southampton on July 22, and the *Saxon,* leaving on July 29, carried the members over the 6,800 miles which separate that port from Cape Town. From there the party traveled by sea or rail to Durban and thence by rail to Johannesburg, making stops at Pietermaritzburg, Colenso and Ladysmith. The scientific meetings were divided between Cape Town and Johannesburg, and four or five days were accordingly spent in each of those towns. After a short visit to Pretoria, the regular program involved a long journey of 1,374 miles to Bulawayo *viâ* de Aar Junction, the only possible all-rail route; on the way, stops of a day or two were made

General View of the Victoria Falls from a Point near the West End.

at Bloemfontein and Kimberley. From Bulawayo five special trains conveyed the oversea party, with the addition of many others living in South Africa, to the Victoria Falls, where a couple of days were spent. On the return to Bulawayo about half the party proceeded direct to Cape Town, whence the regular steamers carried them by the west-coast route to England. The remainder went by rail through Salisbury and Umtali to Beira, where the *Durham Castle* awaited them for the east coast route. On the return journey, Mozambique, Mombasa and Cairo were visited; the presence of plague at Zanzibar and Niarobi upset the arrangements for seeing those two places, but the unexpected block in the Suez Canal enabled the party to spend much more time in Egypt than had been expected. Several members whose duties

THE SPOT IN THE SUEZ CANAL WHERE THE 'CHATHAM' WAS BLOWN UP, causing the Canal to be blocked for over two weeks.

CHARACTERISTIC KOPJES AND A PART OF ONE OF THE RESERVOIRS ON TABLE MOUNTAIN.

called them back early left by the quickest routes from Cairo, many others disembarked at Marseilles, the final port of call, and the remainder proceeded with the ship to Southampton, which was reached on October 24.

The route thus outlined was admirable for seeing as much as possible in the time, thirty-five days, which could be spent in South Africa. But many of those who went out had more specific objects in view than attendance at the meetings or sight-seeing, and arrangements were accordingly made so that any one could deviate from the official route and travel by the ordinary trains. Some went to Durban by the only railway route—through Johannesburg; others omitted Natal altogether and spent the extra time examining the geological and botanical features of Cape Colony and the Orange River Colony; some avoided a great part of the long ride from Johannesburg to Bulawayo by going on 'trek' from Potchefstrom, or from Pretoria, to Mafeking; other parties trekked from Bloemfontein through Paardeberg to Kimberley; and so on. And in each case something new and definite was to be seen or learnt.

Everywhere the arrangements made by the local committees were admirable. When it is remembered that about 360 people from Europe landed in Cape Town and were carried over an immense extent of territory, were lodged and fed everywhere in comfort and without going through any hardships beyond the fatigue caused by such rapid traveling, and this almost without a hitch of any sort, one can not too highly praise the ability and devotion of all those who were responsible for the organization. And it must be added also that it was not only those who kept to the official route who were alone considered. At every place efforts were made to find out what the various members wished to do and, if possible, arrangements were made to accommodate even a small number; alternative excursions were described in printed circulars, previously distributed, and all that was asked was for each member to apply at the committee room for tickets, so that the number joining any particular excursion might be known. At every place where a stop was made each person knew in advance where he or she was to stay, and conveyances and guides were ready at the station so that there should be no delay or confusion. For example, all that was asked of us at Johannesburg was to stand at the windows of our own compartments as the train steamed into the station, and when the train stopped each host was found standing on the platform opposite his guest. Our baggage, previously directed, arrived later in the day, and meanwhile we were driven, first to the committee room, where we made the circuit of a long counter, gathering up handbooks, tickets and mail, and then to our destinations. And so it was everywhere. No matter seemed too small for consideration and preparation. Many of us felt

that perhaps the most striking feature of the tour was the excellence and elasticity of all the arrangements made for our comfort and convenience. If the ability shown by the colonists in this direction is any guide, one should not fear much concerning the administration of the colonies in the future.

In order to secure the attendance of those whose presence was chiefly desired from the scientific side, a fund of over nine thousand pounds was raised, mainly by contributions from the governments of Cape Colony, Natal, the Transvaal and the Orange River Colony, and supplemented by subscriptions from private individuals; this was used to pay the greater part of the expenses of the 'official members.' The governments also issued free passes over railways to all oversea members, and the Rhodesian railways gave a large number for the use of the official party and tickets at half fares for all others. At those places where a stay was made entertainment was provided for the official party, either in private houses or as guests in the hotels; in some places all the members were similarly treated. Most of the excursions were free to those who chose to take advantage of them. It is a privilege to have an opportunity of saying in public what all of us felt, that the generosity and hospitality displayed by the residents of every town far exceeded our utmost expectations, and the kindness which we received is not likely to be soon forgotten. This too in a land only beginning to recover from the ravages of civil war, suffering from a two years' drought, with nearly all its cattle exterminated by disease, and in the height of the most severe financial depression it has known for twenty years.

The official party, numbering about 180, consisted of the president and general officers of the association, the president, a vice-president and a recorder in each section, a number of prominent scientific men, not necessarily officers, and some younger men of promise and ability selected by the general committee. The ladies who accompanied the official members were also attached to the official party. Finally, representatives of other countries were invited to joint as guests of the association. They included Dr. Backlund, from Russia; Professors Beck, Engler, Harzer and von Luschan, Germany; Professor Böhr, Denmark; Professor Cordier, France; Professor Donner, Sweden; Professor Penck, Austria; Professors Kapteyn and de Sitter, Holland; Mr. D. Randall MacIver, Egypt; Professors Macallum, Coleman, J. B. Porter, Canada; Professors D. H. Campbell, H. S. Carhart, W. M. Davis, W. B. Scott and E. W. Brown, United States; and others who were not able to attend.

The general officers of the association for the year are: President, Professor G. H. Darwin (now Sir George Darwin, K.C.B.); secretaries, Major P. A. Macmahon, Professor W. A. Herdman; treasurer,

Professor John Perry. The presidents of the various sections are as follows: A (Mathematical and Physical Sciences), Professor A. R. Forsyth; B (Chemistry), George T. Beilby, Esq.; C (Geology), Professor H. A. Miers; D (Zoology), G. A. Boulanger, Esq.; E (Geography), Admiral Sir W. J. L. Wharton; F (Economic Science and Statistics), Rev. W. Cunningham; G (Engineering), Colonel Sir C. Scott Moncrieff; H (Anthropology), Dr. A. C. Haddon; I (Physiology), Colonel D. Bruce; K (Botany), H. W. T. Wager, Esq.; L (Educational Science), Professor Sir Richard C. Jebb. Amongst others who attended and who are not included in the above lists or in the list of lecturers given below may be mentioned Sir Benjamin Baker, Sir T. Lauder Brunton, Professor John Milne, Dr. J. A. H. Murray, Sir W. H. Preece, the Earl of Rosse, Alexander Siemens, Esq., and Dr. A. Traill, provost of Trinity College, Dublin.

II.

To one accustomed to the rush of the high-speed boats on the north Atlantic, the rows of huddled up and miserable passengers lying in deck chairs, the cold winds and the frequent bad weather, a journey in a mail steamer crossing the equator presents a pleasing contrast. There is a general air of sociability and comfort; sports, tournaments

Some of the Members on the 'Saxon' before the ship left Southampton. In the top row, reckoning from the right, may be seen Prof. and Mrs. Herdman, Prof. Forsyth, Mr. Freshfield, Sir R. Jebb, Sir W. Wharton, Dr. Murray; in the second row, Dr. Haddon, Professor Perry, Sir W. Crookes, Sir L. Brunton, Major Macmahon, Mrs. Darwin; in the third row, the writer, Professor Darwin and Professor Sollas.

and entertainments of all kinds are of daily occurrence; and to these diversions were added in our case, as befitted the character of the company, lectures and discourses on subjects which were generally connected with the countries to be visited. But perhaps the most useful feature of the voyage was the opportunity it afforded for the leisurely discussion of scientific and professional matters and for establishing closer personal relations between men representing various departments of science. It need hardly be said that this was very fully appreciated, especially by those who have their work in places remote from the main centers of intellectual activity.

EUPHORBIA 'SNAPPED' FROM THE TRAIN NEAR DURBAN.

The southern gateway of Africa is an imposing sight as it is approached from the sea. A characteristic feature of the mountains, the table-like formation with high vertical cliffs on one side, has no better example than the huge mass which faces Table Bay, flanked on one side by the conical hill known as the Lion's Head, and on the other by the Devil's Hill. Cape Town lies on the low ground in front of the mountain and one can not see the old and new fortifications guarding the entrance to the docks without remembering its early settlement by the Dutch, its later acquisition by the English and the fact that, until the completion of the railways to Durban, Delagoa Bay and Beira, the story of South Africa is almost contained in that of Cape Town. All through the late war it was the principal port of entry for men and supplies and during that time was a scene of tremendous activity. It is now suffering from severe depression caused by over-speculation

in building and commerce. In spite of the fact that the population of the whole colony is less than 600,000 whites, trading was started after the war on a scale which a white population of twenty millions would hardly have justified. As might be expected in a town of nearly 80,000 inhabitants, Cape Town has the conveniences of a modern city, a fine town hall just finished at a cost of a million and a quarter dollars, a good and plentiful water supply, electric light, extended railway and trolley car lines, and a perfect sewerage and drainage system.

It is not possible for me to warn intending tourists of the troubles caused by quarantine, customs declarations, passports or baggage transport, for all these formalities were dispensed with: we had only to walk ashore in company with our hosts who had come on board the ship to meet us. The first half of the presidential address was delivered by Professor Darwin on the evening of arrival, and the following three mornings were devoted to the sectional meetings. The five days in Cape Town were spent by the different members of the party in different ways, according to their consciences or inclinations. The afternoons were generally free for excursions, and the evenings were fully occupied by receptions or lectures, well attended by both visitors and residents. Many of the geologists were attracted by the opportunity to see the country with their own eyes and obtain data for the discussion of those problems which appear to be peculiar to South Africa. The astronomers were particularly active both in Section A and in afternoon and evening visits to the observatory, the history of which furnishes remarkable examples of devotion to science; under the present director it has not only been equipped with some of the finest and most modern instruments, but has sent forth many valuable contributions towards our knowledge of the heavens. Groote Schuur, the residence of Cecil Rhodes and bequeathed to the colony at his death, was a center of interest as the home of the man 'who thought in terms, not of countries, but of continents,' and nearly every one visited the beautiful house and extensive estate with its large collection of African animals. On the last day some hundred and fifty of the party, guided by members of the Cape Mountain Club and others, climbed up various routes on to Table Mountain and sat down to a lunch provided by the mayor near the new reservoirs which supply the city with water. There were excursions also to various features of interest in the town and its neighborhood, to the De Beers Explosive Works, to the Government Wine Farm at Groot Constantia, to the Admiralty Works at Simonstown, and to the Elsenburg Government School of Agriculture at Stellenbosch.

III.

The southeast coast railway to Durban is as yet incomplete and, to avoid the long railway journey *viâ* Johannesburg, the members left

Cape Town by the *Saxon* on August 18, calling at Port Elizabeth and New London, or by the *Durham Castle,* leaving the following evening and going direct to Durban. The times were so arranged that every one arrived there on the morning of Tuesday, August 22. There is practically only one good natural harbor for ships of large tonnage on the east coast of South Africa—that of Delagoa Bay, in Portuguese territory. Much money has therefore been spent in improving the harbor at Durban by building a long mole and by dredging the shallow channel which leads into a large protected lagoon. It is now possible for the mail boats to go inside and tie up alongside of the quays. One

THE CHIEF PRINCESS OF THE TRIBE WHICH GREETED THE PARTY AT MOUNT EDGECOMBE

was struck immediately on landing by the mixture of the east and the west. Jinrickshas drawn by Zulu boys with their picturesque head-dresses, ordinary two-horse carriages, and electric cars on the trolley system carried the passengers along well-made roads bordered by trees, to private houses and hotels, where they were waited on by Indian servants. Shops of all kinds, a big department store, English churches and chapels, a synagogue, a mosque, three-storied residences, bungalows—all these made it difficult for us to realize that we were in a town which has been British territory since its foundation in 1823. As at Cape Town, there were receptions, lectures and excursions to the more interesting works of nature and man. There were only two days allotted to Durban and the majority of the party spent the greater part of one of them at Mount Edgecombe, some fourteen miles away, where the factory of the Natal Sugar Estates is situated. The company had

issued an invitation for lunch and an inspection of its works, and it had also made arrangements for us to see something of the native element by gathering together over 300 Zulus from the surrounding country. The exhibitions of war and other dances which we witnessed were much appreciated by the ethnologists and photographers. I may mention here that over a hundred cameras were continually employed on all varieties of subjects throughout the whole of the trip. In order that a record of some permanent value may be obtained, it has been proposed to make a selection of photographs taken by those who are willing to lend their negatives and to publish a memorial volume containing the best of the pictures.

THE BRIDE, BRIDESMAIDS AND INDUNA. Mr. Samuelson is standing on the right.

An even more interesting view of native customs was obtained in an excursion to the large Henley reservation near Pietermaritzburg, our next resting place. Maritzburg, as it is generally called, lies in a basin surrounded by hills and is laid out on the Dutch plan, in blocks like an American town, with broad avenues, but with houses which, like most of the residences in South Africa, are only of one story. The reservation is on higher ground and the station is less than ten miles away as the crow flies, but requires a journey of seventeen miles along a railroad with steep grades and sharp curves. The only white man living on the reservation is the permanent undersecretary for native affairs, Mr. Samuelson. By his wish, the marriage ceremony of one of the native chiefs, Mhlola, the head of the Inadi tribe, had

Street in Maritzburg.

A Visitor during a Halt between Mafeking and Bulawayo.

been postponed in order that the association might have the opportunity of witnessing it. The bride, who is to be Mhlola's chief wife, is a 'commoner,' contrary to the usual custom. It is probably the only occasion that a royal Zulu wedding has been attended by a large party of invited guests of the white race. We watched the official part of the ceremony for some three hours; dances, speechmaking and chanting of war-songs not unlike Gregorian chants occupied most of the time. The part of the ceremony which constituted a legal marriage was followed by the presentation of gifts from the bride to her husband's principal female relatives and of symbolical presents to the bridegroom consisting of a lamp, a water jug, basin and soap, a chair and an umbrella. The festivities were to last two or three days, but the members of the association had to leave for other scenes, and they preferred the conventional lunch provided by the residents of the city to the oxen roasted over open wood fires and the Kaffir beer in which the natives delight. This attractive program occupying the only full day spent at Maritzburg prevented many of the visitors from joining in the numerous other excursions which the hospitable residents had arranged. Some idea of our activity throughout the trip may be gathered from my movements on the previous day. Leaving Durban at 8:50 A.M. and reaching Maritzburg at 1:10 P.M., I spent the early afternoon in riding round on the electric cars, seeing the town and visiting the new botanical garden. Then to a garden party at Government House, and after dinner to a lecture on 'Sleeping Sickness,' by Colonel Bruce!

It was a fortunate circumstance that the third volume of the *Times'* history of the Boer war, containing a full account of the operations round Ladysmith, should have been published early in the year. Those who had read it during the outward voyage were able to picture to themselves the various incidents of the struggle as the trains slowly steamed through the area past Estcourt, Frere and Chiveley, to Colenso. An afternoon was spent in climbing the nearer hills of Fort Wylie and Hlangwani, and in viewing the devious course of the Tugela as it threads its sunken bed through the rolling ground lying in front of the round-topped hills which faced the army at Colenso. Stone sangars, but little damaged, are still to be seen on every hand, but the hunters of curios in the shape of bullets and portions of shells had done their work too well long before our arrival, and few relics were discovered. Here the special trains were side-tracked for the night so that the points of interest along the short distance to Ladysmith could be seen by daylight. The residents of this quiet country town lying in a warm hollow on the Klip River had gathered together every available private and public conveyance and drove us to the scene of the most famous incident of the siege, Wagon Hill. This spot, about

three miles from the town, commands it completely, and had the Boers in their determined attempt on January 6, 1900, succeeded in capturing the hill against the desperate defense made by the British, it would have been necessary to retake it at all costs or to evacuate Ladysmith. Another hill of historic interest, Spion Kop, some eighteen miles distant, was visited by a small party who had gone on ahead for the purpose. The town itself bears few marks of the siege. The hole made by a shell in the clock tower of the town hall is still unrepaired, doubtless for the sake of tourists. I noticed the remains of a few of the 'dug-outs' in the steep crumbling banks of the river, and some of the corrugated iron plates which form the walls of a freight shed at the railway station had many bullet-holes in them; they had been evidently used for cover and returned at the end of the siege.

The day at Ladysmith was followed by a night's journey to Johannesburg. The higher veld is reached along a series of heavy grades, frequently one in thirty. There is no attempt to make the line straight; tunnels, embankments and cuttings have been avoided as far as possible to save expense, and the line, especially over rolling plains, closely follows the natural level of the land. Over a thousand feet of height is gained near the border of the Transvaal by a series of zigzags up the side of a mountain; at each of these the line comes to a stop, and the train is reversed up the next portion, and then forward again after another stop. There is apparently no hill around which the line may curve easily in order to obtain the desired height.

IV.

Although Johannesburg has been so often described, I can not pass in silence over this focus of all the later development of the Transvaal and of most of its political difficulties during the last twenty years. Moreover, so many changes have taken place since the war ended and so much misconception still prevails about the conditions there that it is only right and perhaps not uninteresting to record the impressions of one who was anxious to learn the facts and who had various opportunities for obtaining accurate information at first hand. The most striking and noteworthy of these impressions was the absolute openness of everything connected with the mining industry. Not only have very full reports of the working of each mine to be sent in monthly to the government and to the Chamber of Mines, but every new process, every improvement in machinery, every new problem arising, every difficulty occurring in the management of the natives and Chinese, is known or can easily be found out by those living on the Rand. And this is true not only of the residents, but also of any visitors who may wish to learn the facts and will go to the proper sources for them. In our case, the chief desire seemed to be that we should get to know

what the actual conditions were, the bad as well as the good side; it was not a question of searching for information, but of listening to the full answers which an enquiry always produced. In particular, the native and Chinese compounds were visited at all hours both with and without previous notice. My own impressions and those of our party with whom I afterwards talked were the same: that the arrangements for housing and feeding the workers are far better and more complete than we had any idea of, and that the slavery which has been and is still so much exploited in meetings and newspapers of a certain class does not exist. Passes for leaving the compounds during off hours are freely granted to natives and it is only since the commission of crimes outside by a few bad characters that a restriction in this direction has been placed on the Chinese. As one walked about the compounds or in the mines underground the solemn Chinese equally with the light-hearted native readily responded to a word or a smile. 'Tell those who abuse us to come and see things for themselves'—was a frequent remark from the mine officials with whom I talked.

A second striking feature is the change which must have come over the spirit of the so-called 'Outlander' since the conclusion of the war. Formerly, Johannesburg consisted of the business section, the mines with compounds for the natives, and cottages on the mining area for the staff and white workers. During the last three years large suburbs have sprung up with many hundreds of residences surrounded by gardens and young trees, and having every appearance of permanent occupation. If this conclusion is correct, there will be a large settled population within the city area which will take an interest in its future and in the general affairs of the country, in spite of the fact that the majority of the shares of the mining companies are naturally owned in Europe, whence came the money which started them. To this must be added the consideration that nearly all the best work on the mines is being done by comparatively young men who have gone to them with the definite intention of making a living, and who have to use all the ability and energy they possess to rise to the higher positions. There is need now, however, of men of a higher grade, with, if possible, a college education and special training in some one or more of the departments connected with mining.

As might be expected, the town gives every external appearance of being alive. But it presents some curious anomalies. One has not to walk far from the principal streets with fine buildings on either side, shops, offices, clubs and hotels, to reach old shanties which look as if they had been there at the opening of the Rand. Cabs, carriages and automobiles are passing rapidly along the roads (there is no speed limit!), but there is only a single line of slow horse-cars. Instead of a modern sewerage scheme the 'bucket system' is employed. Electric

light is furnished by the municipality, but about two thirds of the current has to be purchased. It is only right to state that all these defects are being remedied at a large outlay of money, and the rates are going up at a speed which may give cause for jealousy in certain cities of the northern hemisphere. It is to be remembered that Johannesburg is only eighteen years old and that for four of these years it had to lie fallow, although it practically escaped damage. I must pass over the many interesting features of the social conditions which the society of the place has evolved.

The scientific meetings of the association, begun at Cape Town, ended with the stay in Johannesburg. Many of the papers naturally dealt with problems and matters relating to South Africa; especially was this the case in the chemical and engineering sections during the latter half of the meeting. I shall not attempt to give any *résumé* of the work done; accounts will be found elsewhere. Professor Darwin's presidential address on the evolution of matter was delivered in two halves, one at Cape Town and the other at Johannesburg. It excited great interest for its own sake and also as continuing the connection between the name he bears and the subject which first gave it world-wide fame. The many illustrated evening lectures on a great variety of subjects were a special feature throughout the tour; some of them had been prepared at the cost of much time and money, and, judging by the attendance, were very fully appreciated by those who heard them. The list included the following: 'W. J. Burchell's Discoveries,' by Professor Poulton, and 'Surface Actions of Fluids,' by Professor Vernon Boys, in Cape Town; 'Mountains of the Old World,' by Mr. Douglas Freshfield, and 'Marine Biology,' by Professor W. A. Herdman, at Durban; 'Sleeping Sickness,' by Colonel D. Bruce, and 'The Antarctic Regions,' by Mr. H. D. Ferrar, at Maritzburg; 'Distribution of Power,' by Professor Ayrton, and 'Steel as an Igneous Rock,' by Professor J. O. Arnold, at Johannesburg; 'Fly-borne Diseases, etc.,' by Mr. A. E. Shipley, at Pretoria; 'The Milky Way and the Clouds of Magellan,' by Mr. A. R. Hinks, at Bloemfontein; 'Diamonds,' by Sir W. Crookes, and 'The Bearing of Engineering on Mining,' by Professor J. B. Porter, at Kimberley; 'Experimental Farming,' by Mr. A. D. Hall, at Mafeking; 'Rhodesian Ruins,' by Mr. Randall MacIver, at Bulawayo.

(*To be continued.*)

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WITH THE BRITISH ASSOCIATION IN SOUTH AFRICA.[1]

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V.

PRETORIA, the capital of the Transvaal, presents the greatest contrast to its ambitious neighbor forty-five miles away. Although it is 4,500 feet above sea level, nearly the average of the rest of the colony, the hills which surround it give the impression of a rather low situation, but it loses nothing from the numerous blue gums, willows and other trees which are to be found everywhere in the city. The chief interest to a visitor naturally arises from its past history and its connection with the last president of the South African Republic. The fine Parliament House and Law courts are imposing beside the many one-storied houses which constitute the greater part of the town; near-by are Kruger's house and the church which he attended. In spite of the fact that Pretoria was down on the program only as an excursion from Johannesburg, its residents were not behind those of other towns in making hospitable arrangements for such as were able to take advantage of them; perhaps the most fully appreciated was a cross country 'Trek' to Mafeking which will presently be described.

A few miles to the north lies the new Premier Diamond mine, a wonderfully rich pipe of yellow, red and blue ground which a short time ago produced the largest stone ever discovered. It is less than three years since the place was bare rolling veld; now there is a hole over seventy acres in extent and forty to sixty feet deep surrounded by machinery and a high barbed-wire fence. The statistics given to us showed that already more than a million carats of diamonds have been taken out and that test borings down to a thousand feet exhibited ground similar to that near the surface. An invitation from the management to lunch and to an inspection of the mine was accepted by at least a hundred and fifty members. It was amusing to be with and to watch the party, guided by Mr. Cullinan, the original discoverer, and his staff, wandering through the diggings and examining the ground, evidently in the hope of discovering another Cullinan diamond; and later crowding round the tables on which the concentrates were spread for examination—the stage where mechanical treatment ends and hand labor begins—and picking out a few small stones. This final process is shortly to be replaced by a mechanical one based

on the fact that the diamond seems to be the only stone which will stick to a bed of grease when a pan of 'concentrates' (the remainder after all the earth and lighter material have been washed out) is passed over it with a properly adjusted flow of water.

A cross country trek from Pretoria to Mafeking seemed to offer greater attractions than the 882 miles of rail which separate those places, in spite of the fact that Bloemfontein and Kimberley would thus be omitted. At present there are no rail connections between the two trunk lines going north from De Aar Junction (which lies just south of the Orange River), although Klerksdorp, the terminus of a branch line from Johannesburg, and Mafeking, on the Cape-Bul-

SPOT (in the bank near the telegraph post) WHERE THE FAMOUS CULLINAN DIAMOND WAS FOUND.

awayo railroad, are only 93 miles apart. These connecting links are of course valuable for opening up the country through which they pass, but it is difficult to get a return on the capital laid out where the chief traffic is to and from the coast and not an exchange between inland centers. But it is hoped that the time is not far distant when the farms in this district may furnish regular supplies to the large towns and make them independent of imported food produce. Railways are already projected from Pretoria to Rustenburg, sixty miles to the west; and also from Klerksdorp to Fourteenstreams which lies on the Cape-Bulawayo railway, 140 miles to the southeast, thus making the first connection between the two trunk lines north of their branching point.

VI.

The route chosen for the journey to Mafeking by road lay through 180 miles of some of the most fertile districts of the Transvaal and included nights spent at the small towns of Rustenburg, Zeerust and Ottoshoop. Two stage coaches, each capable of carrying eighteen passengers with baggage, and a large ambulance wagon were provided for the accommodation of the party which, with guides and leaders, numbered thirty. These coaches have of course been gradually supplanted by railroads where there was sufficient traffic to justify a regular service, but they are still in use in Rhodesia. As the illustration

AN IRRIGATION DAM AND TRENCH ON MR. GINSBERG'S FARM. (Photo lent by Mr C. G. Darwin.)

shows, they are of the Concord type and indeed those which actually conveyed us were built in the United States. Six pairs of mules were harnessed to each coach. We were accompanied throughout by Messrs. H. H. Hewson, W. D. Sievwright and G. W. Herdman of Pretoria, and it was mainly owing to their care and thoughtfulness for our welfare that no serious mishap occurred during the six days' trek. The magistrate of each urban district through which we passed also joined the party, while it traveled through his territory, and much was learnt of the land and its people from these gentlemen and from residents whom we met along the route from time to time. The limits set to this article forbid more than a brief account of the general impressions gained. It must suffice to mention that the first night we camped

Crossing a Drift.

luxuriously on Mr. S. Ginsberg's farm as his guests, the camp having been provided by the Royal Engineers, and the next morning wandered over the farm inspecting the experimental growing of tobacco and oranges and an irrigation trench, some two and a half miles long, carried round the side of a hill. Another night we slept on the open veld wrapped in blankets and rugs. Our experience of the hotels at the three towns mentioned above was a favorable one; they have nothing to lose by a comparison with those in places of a similar size in either Europe or America.

The government is carrying on the work of improving the main roads in farming districts by building bridges over the deeper 'drifts' (fords where the rivers can be crossed), by metalling the surfaces, and by digging side trenches to carry off the torrential rains during the wet season. This is in line with the policy of developing the agricultural possibilities of the Transvaal through an increase in the facilities for getting the produce to a market. But the difficulties of raising it are many. The cattle have been nearly exterminated by war and disease; to prevent the spread of the latter in future the farms are being accurately surveyed and surrounded by barbed-wire fences. The raising of crops with any regularity seems to require expensive schemes of irrigation and the construction of dams to store the water, but it is by no means certain that these schemes can be made to pay their cost. Tobacco growing has long been fairly successful in some parts and the leaf finds a ready sale. Some fruits, especially oranges, can be also

grown with success where the farmer has sufficient capital to await the time necessary to get a crop, but the cost of transportation prevents export in competition with the fruits produced in other parts of the world. It is well known that a great effort has been made by the government to get the Boers back on their farms, and we saw one example of this in the new houses which have been built near the roofless walls of every old one that we passed. For the Dutch settler is in general the only class which has so far succeeded in extracting a living out of the land, partly owing to his few needs and his content with meager surroundings, but he is in some ways an obstacle to development by his constitutional dislike to any alteration of the methods handed down to him from his ancestors.

On every side were to be seen evidences of the long-continued guerilla warfare; block houses perched on the hills, sometimes in long rows a mile or two apart, at other times in isolated places; an occasional area covered with rusty tin cans showing where a concentration camp had been situated; skeletons of cattle and mules along the roadside; an acre of the whitened bones of oxen, the scene of the destruction of a convoy caught in a trap. Many of the pleasures and troubles of trekking were experienced. The night under the open sky on the veld, various breakdowns and minor accidents, the hot noon suns and cold starlit skies, the clouds of red dust raised by the mules—all combined to give some idea of that fascination for traveling in Africa which has so often been the theme in stories of fact and fiction.

TREE IN RUSTENBURG UNDER WHICH THE LATE MR. KRUGER PREACHED HIS FIRST SERMON TO THE BURGHERS.

Mafeking has little of interest for the ordinary sightseer and nothing remains of its spectacular siege except a few banks on the flat plain showing where the trenches had been placed. The native 'staadt' contains some five thousand blacks living in huts and houses of sun-baked bricks and plaster, with occasional corrugated iron roofs. The special train only stopped here long enough to gather up those who had come by road from the Transvaal. All the following day was spent in running along over the brown veld, sometimes flat and bare, sometimes covered with thick bush, but generally rolling country dotted with trees and intersected here and there with the dry beds of

AN INCIDENT OF OUR 'TREK.'

streams. At this season of the year the ground has become parched under the hot sun and long coarse dry grass covers the whole face of the country. A tree with a straight trunk is rarely visible and the twisted branches were devoid of foliage except where parasitic growths, frequently species of misletoe, showed their bright green stems. All the way from Durban to the end of our ride, grass fires, started by the farmers to clear off the ground before the rains, were visible and often made the nights picturesque as they slowly burned their way in long lines over the plains and hills.

The standard South African railway gauge is forty-two inches, fourteen and a half inches less than the ordinary one. This is probably an economical width for the present needs of the country, but it introduces difficulties in the construction of comfortable sleeping ac-

commodation. The present type of car used on the Cape government railways has a very narrow side corridor from which open compartments, each containing four berths, two upper and two lower, transverse to the length of the car. These berths are rather short for one a little over the average stature, and the lavatory accommodation is somewhat limited. But the dining cars provide excellent meals at two dollars a day and this in a country into which much of the food is at present imported must be considered very moderate, especially north of De Aar Junction. The new cars on the Natal railways are, however, of a much more roomy and convenient type. It was in special trains made up from these cars that the majority of the members of the association was to spend most of the two weeks following the departure from Johannesburg. The life on board was not uncomfortable, and there was plenty to interest in the views which successively passed before us as we steamed along at fifteen to thirty miles an hour, or in discussions on what we had seen and heard. Then at every stopping place, and these were not infrequent for taking water or coal, the zoologists swarmed from the train with nets and snared every insect within a radius of two hundred yards, and the geologists with their hammers gathered in treasured specimens of rocks. The engineer became skilful in solving the problem of gathering up the passengers and not wasting time in waiting for the laggards, by steaming so slowly out of the way side stations that any one not more than a hundred yards from the train when it started could easily get on board.

VII.

Bulawayo, the principal town in Rhodesia, exhibits strongly the large ideas of Cecil Rhodes and his confidence in the future. Laid out in blocks, with streets far wider than one finds even in the most modern towns, its principal buildings in the center near an immense market square, Bulawayo is prepared for development to an extent which seems to be out of proportion to its needs for many years to come. At the present time there are many inconveniences in having the town so widely spread out, and the expense of running it is not small. Except in the center, one can drive along roads with name posts at every corner, but not to be traced otherwise than by wheel tracks in the yellow dusty ground. Rhodes's house, presented by him to the government, is situated on a hill three miles from the town and is connected with it by a perfectly straight and broad road planted with a double avenue of trees. A better method could hardly have been devised for enhancing the dignity of the approach to his residence or for striking a note in his character—the direct route to his objective and a well-marked way for those who should follow in his footsteps.

THE PRINCIPAL STREET IN BULAWAYO.

The poetic side of his nature is shown in his choice of a final resting place. From a point twenty miles along the railway south of Bulawayo a branch line runs towards the Matopo hills where he had a large estate. A drive of eight miles from the hotel at the terminus leads into wild scenery along gradually ascending valleys, past large enclosures containing wild animals and through a park which is being continually improved by the planting of trees and all kinds of flora. On either side the road is flanked by hills which seem to have been built up by Titans who piled up rocks and boulders in every conceivable position, perching them on the tops and sides of smooth turtle-back shaped rocks five hundred feet high, or dropping them on the plain and covering them with bushes and trees. As the 'Roof of the World' is approached, the carriages are left and a footpath ascends gradually over smooth rock on to the flat top of one of the highest of the hills—the 'World's View.' On this spot, enclosed by a circle of boulders some fifteen or twenty feet high, is placed the grave dug out of solid rock and covered by a plain slab bearing only the simple inscription, 'Here lie the remains of Cecil Rhodes.' A not unpleasing contrast is afforded by an elaborate monument nearby erected to the memory of Major Wilson and his comrades who fell at the Shangani River on December 4, 1894.

This large estate was owned by Cecil Rhodes and was left by him under the care of the trustees for the benefit of the public, full direc-

tions being given in his will for improving the property with the help of funds which he designated for the purpose. One can not travel through Rhodesia or indeed through any part of South Africa without feeling how strongly the ideas of this one man have dominated and still largely determine the development of the country. Whatever we may think of his career, we are forced to admit that the reverence felt for him and his opinions by those who worked with or under him mark him out as a personality of unusual force. He inspired too an enduring belief in the future of Rhodesia, and this in the face of almost every difficulty that a new country has to undergo. Against the condemnation of some of his actions at the bar of public opinion is to be set the opinion of those who knew him and who believe that he acted consistently with a high standard of his own and that at his early death the British Empire, and perhaps the world, lost one who might have achieved a foremost place in the history of nations.

The Victoria Falls on the Zambesi river lie 282 miles to the northwest of Bulawayo. The curious box-like formation into which the water drops with the lip over a mile long and the opposite ground on the same level and not more than 150 yards away, gives unusually fine points of view and permits every part of the falls to be seen. When the water is low, as was the case at the time of our visit, one can see down to the bottom of the chasm 400 feet below; or cross over to the islands above and look down into the depths from the uncovered rocks with the water tumbling down close by. The river leaves the 'box' by

HOTEL AT THE VICTORIA FALLS.

a narrow opening some distance from the middle of the long opposite edge, and pursues its way through a deep gorge which winds to and fro like the strokes of the letter W several times repeated, showing clearly the successive stages by which the river bed has burrowed its way through the country. The 'rain-forest,' a thick mass of trees and undergrowth, and the Palm Kloof, a ravine leading down to the bottom of the gorge, are kept moist by the shifting masses of spray. Above the falls, the banks are clothed with tropical vegetation and the long reaches of apparently calm but swiftly flowing water show little of the many hidden dangers which small craft passing along them must avoid. The marvels of nature are perhaps equalled by those of civilization.

THE 'WORLD'S VIEW.'

Ten years ago not more than thirty white men are known to have visited this spot since its discovery in 1855 by Livingstone, and last year it was connected with Bulawayo by rail. The line has now been carried over the gorge just below the falls by a bridge 650 feet long and 350 feet above the water, finished last spring; it is being continued now to Lake Tanganyika, and had in September reached a point 160 miles from the Falls towards this objective. Perhaps the greatest marvel of all, as Professor Darwin remarked in opening the bridge to passenger traffic on the morning of September 12, was that his speech on that occasion should appear in full in the London afternoon papers of the same day.

Tourists will now find no difficulty in reaching the Falls nor will

they need to expect discomfort while staying there. The hotel, about a mile from the principal points of view, supplies food and lodging on much the same scale as those in other parts of Rhodesia. Perhaps the greatest satisfaction on arrival is the absence of any feeling of disappointment, however much one may have heard or read of the beauty and magnitude of the falls, and civilization has so far done nothing to spoil the views. Mr. F. W. Sykes, who has been appointed conservator by the Chartered Company, has constructed paths so that visitors may approach every point of view and enjoy the scenery without the encumbrance of a hired guide. The new bridge rather adds to the effect than otherwise: as one descends to the bottom of the gorge amidst the

THE PRESIDENT OPENING THE BRIDGE.

trees and undergrowth in the Palm Kloof, its graceful arch gives the eye a resting place near the top and assists one to appreciate the height of the rugged vertical cliffs. I understood that the power house is to be constructed at the foot of the second bend of the gorge, the water being conveyed from above the falls by a tunnel or canal, so that nothing of it will be visible from the usual points of view. It is intended that no new buildings shall be placed on the general level of the land within a radius of a few miles and plans are even in existence for moving the present hotel further away. As to the available energy for commercial purposes, the latest estimates give a minimum of 300,000 horse-power at low water as against 5,000,000 at Niagara; but after the rains there would be many times this amount so that even if the minimum

RAINBOW SEEN LOOKING ALONG THE FALLS FROM NEAR THE WEST END.

PART OF THE 'MAIN FALL' FROM LIVINGSTONE ISLAND.

should be wholly employed, the spectacle at full flood would not be very seriously affected. The opinions of the residents as to the best time for a visit are divided. Some preferred August and September when the water is low and the air comparatively clear of mist; others recommended January and February for seeing the huge masses of water which then cover nearly the whole width of the lip but which can only be seen in glimpses as the spray shifts about.

VIII.

On the return to Bulawayo, the east and west coast parties separated, the latter going direct to Cape Town and thence home. The rail journey to Beira on the east coast was broken at Salisbury and Umtali. Both of these towns are situated in or near gold-bearing districts. The region is interesting too to ethnologists on account of the ancient ruins to be found at Zimbabwe and elsewhere, but it was sad to learn that all the later evidence so far obtained has destroyed any connection between Rhodesia and the land of Ophir. The party, now reduced to two hundred, was entertained at Salisbury and Umtali by the residents to lunch; and similar hospitality was shown by the governor, the Portuguese officials and the Mozambique Company at Beira. Our debt of gratitude to these three towns is the greater for the trouble and expense to which the small number of residents had put themselves, although our stay in each had to be limited to only a few hours; there was no chance to make even the small return in our power by giving lectures or by learning, except in conversation, of the development of the districts round these recent settlements.

A few concluding words on Rhodesia must suffice. The details of its administration and development by the British South Africa Chartered Company are to be found in the published reports and circulars of the company. As to its possibilities, I can only give here, with all reserve, my own opinion formed on what I saw in the rapid journey or learnt in several conversations with various officials and others. In its general characteristics, the country does not appear to differ greatly from the Transvaal. But it seems to have rather better advantages. Its soil is perhaps more fertile, its rains more certain and droughts less frequent. The mineral wealth is considerable; there are excellent coal seams, a rich copper mine and, if the present prospects are fulfilled, valuable gold fields. A magnificent river flows through the country, adapted at the Falls to furnish power for all purposes in the driest season and possibly available in the future for irrigation if necessary. An unbounded enthusiasm and belief in its future amongst those who are administering its affairs there are not amongst the smallest of the assets of Rhodesia.

The 'Durham Castle' left Beira on September 17. A brief call of

a few hours was made at the low island built of coral on which Mozambique stands. The town is picturesque with its square topped houses and walls washed a bright red, yellow and light blue, the native huts of bamboo thatched with palm leaves, and the numerous palm trees growing everywhere. A stay of a day and a half allowed us to see Mombasa, to make purchases in its native bazaars, and to take a journey by train to Mazeros, fourteen miles up into the country. The town is close to the equator and we saw luxuriant tropical vegetation, cocoanut and other species of palms, and the huge squat trunks of the baobab—a pleasing contrast after our long experience of the dried-up veld. Leaving there, eleven days of burning sun and hot stifling nights in the Indian Ocean, across the gulf of Aden and up the Red Sea whose waters one day showed a temperature of 92° Fahrenheit, brought us to Suez. After a week in Egypt necessitated by the block in the Canal, the ship left Port Said for Marseilles where many landed in order to reach England rapidly. The remnant, passing through the Straits of Gibraltar and crossing the Bay of Biscay, disembarked at Southampton on October 24.

The one sad incident which occurred during the tour was the illness and death of Sir William Wharton, at Cape Town, after our departure from Beira. His work and scientific attainments will find a more fitting record elsewhere. Those who had learned to know him as a fellow-traveler can readily understand and sympathize with the sense of loss experienced by his family and many friends. As I revise these lines comes the news of the death in Cambridge of another member of the party which will not be less severely felt, Sir Richard Jebb, perhaps the most distinguished scholar of his day and a leading authority on educational questions. One rarely talked with him without drawing something interesting from his great store of knowledge and he added much to the success of the meeting and the pleasure of the voyages by his presence amongst us.

IX.

It is almost impossible to sum up in a few sentences the wealth of impressions received during the five weeks in South Africa and the subsequent brief visits in East and North Africa. A 'gigantic picnic,' as Professor Darwin characterized the tour in one of his speeches, it truly was; but it was also a 'scientific picnic' with wonderful opportunities for profit to those who wished to take advantage of them. The various handbooks, specially prepared for us, on matters connected with the colonies, the arrangements made for seeing everything without waste of time and with the minimum of trouble, the way in which all the people put themselves at our disposal whether for showing the country or for telling what they knew—all helped to make the experi-

ence no ordinary one and enabled us to carry away facts and ideas which could hardly have been obtained in a much longer period. To those who are accustomed to travel in Europe and who have money and leisure for four months or more away from home, a visit to South Africa is to be highly recommended. The steamers, while not equipped with the excessive luxuries of the most modern North Atlantic boats, are comfortable and sail over waters which are rarely disturbed by storms or gales. The long distance trains are at least as good as those in Europe, and hotels, accustomed to cater for English people, will be found everywhere. The cost of such an expedition extended

A STREET IN MOMBASA.

over four months need not exceed fifteen hundred dollars per head, including passage money.

Finally, what should South Africa hope to receive in return from those who have accepted her hospitality? An increased sympathy with her people, a better knowledge of their struggles in developing the resources of the country, and an attempt to put an end to the long career of misrepresentation which has been pursued by many public bodies and private individuals in the mother country, doubtless. But there is more. The colonies are not lands where the agriculturist can simply sow his seed and watch his crops grow; where the rancher can stock his farm and await the increase; where the planter puts in his trees and leaves them until the harvest is ripe; where the miner has only to dig out the gold and grow rich quickly; or where the trader can take his goods and calculate his profits beforehand. Irrigation,

and, in many cases, fertilization of the soil, are generally necessary for obtaining a moderate crop; cattle must be protected from the parasites and diseases which carry them off wholesale; the planters must make many experiments to find suitable trees and then discover a market for his fruits; gold digging can only be made to pay by companies with large financial resources employing the most modern scientific methods for the extraction of the metal; and the trader is handicapped by the cost of transportation and the small demand for his goods. These are some of the problems which the colonist asks his visitors with their store of knowledge to help him to solve: he needs every device which science can furnish to enable him to exist. Further, his land has been

INSIDE THE MOHAMMEDAN UNIVERSITY IN CAIRO. (Photo lent by Mr. C. G. Darwin.)

lately rent by civil war, and two white races with totally different ideals must live side by side working together for the common good: the black races, far outnumbering the settlers, present problems at least as difficult as those we have to deal with in the United States. He asks too for help in building up schemes of education for both black and white, and these schemes must include primary and secondary schools, colleges and universities for the study of the humanities and pure science, and, what is perhaps more important than all for the prosperity of the colonies at the present moment, institutions where elementary and advanced technical education in all its branches can b obtained. If any help is forthcoming towards the solution of the questions, South Africa will feel well repaid for her hospitality a will consider that the visit of the British Association to her shores l not been in vain.

Die Biologie der Griechen.

Von

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a. o. Professor der Zoologie an der Universität Basel.

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Herrn Professor Ernst Haeckel

bei Anlass seines 70. Geburtstages

16. Februar 1904

ergebenst gewidmet

vom Verfasser.

Basel, den 16. Februar 1904.

Hochverehrter Herr Professor!

Gestatten Sie, daß an Ihrem siebenzigsten Geburtstage der Unterzeichnete sich unter den Glückwünschenden einstellt und Sie bittet, die Widmung dieser Schrift annehmen zu wollen.

Mein besonderer Grund zu dieser Bitte liegt in Ihren Verdiensten um die systematische Neugestaltung der Biologie, wie Sie sie in der „Generellen Morphologie" und später in der „Systematischen Phylogenie" mit weitem Blick auf das Gesamte unserer Wissenschaften und mit sicherer Hand im Einzelnen durchgeführt haben. Niemand kann diese Ihre Werke anders als mit dem Gedanken aus der Hand legen, daß sie denen der hervorragendsten biologischen Systematiker ebenbürtig den großartigsten Versuch der Gegenwart darstellen, die Erscheinungen des organischen Lebens von einem Gesichtspunkte aus zu durchdringen und ihr Wesen mit den Mitteln des menschlichen Geistes künstlerisch nachzuschaffen. An diesem Punkte berührt sich aber Ihr wissenschaftliches Werk nicht nur mit dem der größten Biologen, sondern es legt auch neben dem von Winckelmann und Goethe aufs Neue Zeugnis ab für den ἱερὸς γάμος deutschen und hellenischen Geistes.

Empfangen Sie daher bei diesem Anlasse den Dank für reiche Anregungen, die ich wie so viele Andere aus Ihren Schriften geschöpft habe; damit verbinde ich die Hoffnung, von Ihnen am allerehesten verstanden zu werden, wenn ich aus dem Studium der Entwicklungsgeschichte unserer Wissenschaften für diese selbst eine bewußte und vielversprechende Erweiterung und Vertiefung erwarte.

Ihr ehrerbietigst ergebener

Rudolf Burckhardt.

Hochverehrte Anwesende!

Auf dem internationalen Zoologenkongreß in Berlin hatte ich vor zwei Jahren einen Studienfreund wiedergesehen, mit dem ich seinerzeit im Leuckartschen Laboratorium zu Leipzig gearbeitet hatte. In der Hast des Kongreßlebens war keine Zeit dazu geblieben, daß wir mehr als uns wiedererkannt hätten, und da mich nichts daran hinderte, folgte ich auf der Heimreise der herzlichen Einladung Reinholds, ihn in seiner Universitätsstadt zu besuchen, damit wir uns aussprechen könnten.

Ob wir uns wohl noch verstehen würden? So manchen Kameraden hatte ich nach langer Pause wiedergesehen und gehofft, mich mit ihm einer gemeinsamen Unterhaltung zu erfreuen. Wie oft schon war ich enttäuscht worden, den einen immer noch auf demselben engen Arbeitsgebiete vorzufinden, dem seine Dissertation angehört hatte, zu sehen, wie er alle Erweiterung des Horizontes durch Aufnahme neuer außerhalb gelegener Stoffmassen und Gedanken ablehnte und stets denselben Faden fortspann, den der Zufall und das Interesse seines Lehrers in ihm angesetzt hatte. Man verstand ihn nur nicht; aber über dieses von ihm entdeckte Entwickelungsgesetz, dem er sein Leben widmete, ließen sich nicht nur Bogen, sondern Bände füllen und wenn er einmal durch einen Glücksfall hinaufgetragen werden sollte, so würde eine ganze Schule daran zu arbeiten haben, seinen Gedanken weiter zu verarbeiten.

Ein anderer war dermaßen mit Berufsgeschäften überhäuft, daß auch ihm keine Zeit zur Umschau übrig geblieben

war und er, mühsam seinen Verpflichtungen nachkommend, es ablehnen mußte, nicht notwendige Studien, „Unnötiges“, zu treiben. Besoldet war er ja; gewissenhaft und pflichteifrig versah er sein Museum; mit den Jahrzehnten mußte auch er avanzieren und zu seiner verdienten Anerkennung gelangen. Wie war es wohl meinem Freunde Reinhold ergangen? Hatte er die hohle Gasse hinauf- oder hinabsteigen müssen? Nun, wir werden es ja sehen.

Mit solchen Gedanken beschäftigt, entstieg ich dem Schnellzug, und pünktlich, wie versprochen, empfing er mich am Bahnhof. Er versicherte, er habe sich für den Nachmittag frei gemacht und sein Plan sei, wir wollten sofort nach Tisch sein Laboratorium aufsuchen; bei der Hundstagshitze sei man nirgends besser aufgehoben als in diesem Halbkeller, der im Winter zwar ein elendes Malepartus sei, im übrigen aber prachtvolles Nordlicht zum Mikroskopieren besitze. Ich willigte in alle Vorschläge gerne ein; ist es doch gerade die Kunst des experimentellen Historikers, das Opfer der Beobachtung sich in vollem Behagen ausgeben zu lassen, und Opfer der Beobachtung sind mir, seit ich die Geschichte meiner Wissenschaft erforsche, so viele, auch die besten wissenschaftlichen Freunde geworden. Nur aus der lebenden Wissenschaft und den psychologischen Voraussetzungen ihrer Vertreter schöpfen wir die Kraft, Analogie und Widerspruch der uns nur überlieferungsweise bekannten Vergangenheit sowie die Entwickelungsgeschichte unserer Forschung, zu deuten.

Wir hatten uns niedergesetzt und ich sah mich im Laboratorium meines Studienfreundes um. An Geräumigkeit ließ es nichts zu wünschen übrig. Auch nicht an Ausrüstung. Neben den nötigsten Requisiten standen einige der rostigen Degeneration ihres Skelettes verfallene Aquarien. Mehrere Mikrotome neuester Konstruktion unter Glasgehäusen, wertvollen Sammlungsobjekten gleich, ein elektrischer Ofen für Einbettung in Paraffin, der große mikrophotographische Apparat von Zeiß und die Kohlensäureflaschen, deren Inhalt zum Gefrieren von Schnitten zu dienen hatte. All das verriet den modernsten Betrieb eines Mikroskopikers.

„Kennst Du schon die neueste Verbesserung des verschiebbaren Objekttisches; ganz wundervoll namentlich bei

Immersion; keine momentane Verschwommenheit des Bildes mehr während der Verschiebung selbst. Sieh nur einmal her.“ Ich mußte mit einiger Beschämung gestehen, daß ich bis jetzt noch ohne dieses Hilfsmittel ausgekommen sei, überhaupt ohne verschiebbaren Objekttisch.

„Nun werde ich Dir also gleich zeigen, welch brillante Bilder Du erhältst; diese Technik ist einfach großartig; so bist Du doch absolut sicher, dieselbe Bindegewebsfaser nie aus dem Auge zu verlieren.“

Mein Freund war nämlich, wie Sie sehen, Histologe und seit Jahren der Struktur und Entwickelung der Bindegewebsfibrille immer mehr auf der Spur. Das war seine Domäne; hier war er Autorität. Eine Kontroverse, in die ihn ein unbequemer Nebenbuhler verwickelt hatte, da die Arbeit Reinholds aus Versehen einmal einen Tag zu spät in die Zeitschrift gelangt war, hatte nach der vollen Überzeugung Reinholds mit der Abschlachtung des Gegners geendet. Er hatte ja schon fünf Jahre der Übung und Betätigung auf diesem schwierigen Gebiete hinter sich, als der andere erst anfing. Der Vorsprung war nicht mehr einzuholen. Ein Glück, daß alles so abgelaufen war; eine Niederlage hätte Reinhold in seiner Karriere schwer schädigen können, da sich gleichzeitig mit ihm ein Ornithologe des Museums zur Habilitation angemeldet hatte, „ein Mensch, der nicht einmal die Anatomie eines Vogels kannte, geschweige denn von Histologie eine Ahnung hatte“.

Mein Freund nahm mein Stillschweigen wahr. Nachdem ich mich von der Vortrefflichkeit seiner Bindegewebspräparate überzeugt hatte, und da mir weiter keine technischen Vervollkommnungen von Instrumenten zu zeigen waren, schlug er vor, wir wollten einen Rundgang durch das Institut antreten. Es sei ein günstiger Moment, kein Mensch da; es wäre unangenehm, dem Chef zu begegnen, mit dem er sich zwar recht gut stehe, der aber die fatale Eigenschaft habe, Gäste um ihre Meinung über seine Präparate zu fragen und sie nicht mehr loszulassen. Wir machten uns also auf, traten den üblichen Rundgang an und besichtigten das glänzend eingerichtete Institut. Als wir in Reinholds Zimmer zurückgekehrt

waren, fragte er: „Sag' einmal, was machst Du eigentlich? Noch immer unverheiratet? Noch immer Extraordinarius? Du hast mir ja auch Arbeiten geschickt; aber, offen gestanden, gelesen habe ich nichts. Um Gotteswillen, woher soll einer die Zeit nehmen, nur die histologische, nur die Literatur über Bindegewebe und Mesoderm zu bewältigen? Wo soll es noch hinführen, wenn es so weiter geht, wie in den letzten zehn Jahren? Ja, ich begreife nicht, warum der Zudrang zu unserem Fach stets noch im Wachsen ist? Dabei ist makroskopisch bekanntlich nichts mehr zu machen, alles ist ausgeschöpft und in der Histologie sind wir auch bald an der Grenze!"

Trostlos und leise klangen die letzten Worte aus. „An der Grenze" wollte mir ein Echo von den Wänden des großen Raumes zurücktönen. An der Grenze schien mir der Sprecher selbst. Starr ruhte sein Blick auf dem mächtigen Mikroskop und seine müden Augenlider fielen herunter. War nicht eben noch seine Frage nach meiner Beschäftigung unter dem Ausbruch seiner Verzweiflung über den Betrieb der Wissenschaft erstickt? Wollte er wirklich wissen, wonach er fragte? Konnte ich den Ermüdeten wecken und ihm erzählen, wie und womit ich mich seit meiner Studienzeit beschäftigt habe? Nein, er konnte mich ja nicht verstehen, bei dem wachen Bewußtsein eines Mikroskopikers sicher nicht. So sollte er in süßem Traume wenigstens erfahren, worin seine Freudlosigkeit und das ebenso ehrliche wie unbefriedigte Ringen so manches modernen Biologen seinen Grund hat. Im Unterbewußtsein, von den Zwangsvorstellungen seines Berufes frei, so sollte er wissen, welches Verhältnis des Forschers zu seinem Objekte unserer Wissenschaft zum Leben verholfen hat und stets eine neue Quelle fruchtbarer Anregungen bleiben wird. Mein einst so fröhlicher und lebensvoller Freund sollte, hoch über Zeit und Raum erhoben, schauen, wie geniale Menschen eine biologische Wissenschaft schufen, die, aus der Fülle des Lebens geboren, zum höchsten Berufe bestimmt ist, zur Sklaverei dem Sklaven wird, dem Freien aber zur Freiheit.

„Jetzt landen wir an der Insel Kos", flüsterte ich, als Reinhold nicht mehr erwachen konnte. Die monotonen Tropfen des Wasserhahns verwandelten sich [illegible] chläge und die von sechs Ruderern geführte Ba[illegible] wohlgeschütz-

ten Hafen der kleinasiatischen Insel ein. Ich faßte Reinhold bei der Hand und ließ ihn mit mir hinaufwandern nach der Stadt, die, von sanften und duftigen Wellenlinien des Gebirges umragt, über dem steilabfallenden, nordöstlichen Vorgebirge sich hinzieht. Die Sonne stieg über dem Höhenzug von Halikarnaß empor, und der Morgen brach an, da wir die Gäste des Asklepios sein würden. Bald standen wir auf der Terrasse mit dem weiten Ausblick über das Gestade Joniens und deutlich wie auf der Landkarte trat die seltsam gegliederte Küste aus der weichenden Dämmerung hervor.[1]) Über die breite Freitreppe stiegen die Patienten herunter, die am Vorabend zum Tempelschlaf zugelassen waren. Der sie begleitende Priester fragte Reinhold nach unserem Begehren und da ich ihn unterwegs von meinem Vorhaben unterrichtet hatte, ihn zunächst mit der Naturforschung der koïschen Mediziner in Berührung zu bringen, antwortete er dem Priester traumverloren: „Eine Vorlesung wollten wir hören“. Aber der Graubart erwiderte: „Das, junger Freund, gibts bei uns nicht. Wer um der Menge willen offen redet, beginnt kein rühmliches Unterfangen“.[2]) Reinhold blickte mich verlegen an. Ich aber schwieg, um die Heiligkeit des Ortes mit voller Macht auf ihn einwirken zu lassen. Dann führte ich ihn nach der Stadt in die Hauptstraße, wo Polybos, der Schwiegersohn des großen Hippokrates wohnte. „Hier lies, bis er kommt“, sagte ich und drückte ihm eine Rolle in die Hand, nachdem uns der Sklave auf meinen Wunsch in die Bibliothek des Herrn geführt hatte.

„Denn auch das Gehirn differenziert sich wie die übrigen Körperteile und entwickelt sich zu einer Art von Blüte.“ „Es ist doppelt beim Menschen, in der Mitte von einer Scheidehaut getrennt, auf seiner Erkrankung beruht die Epilepsie.“ „Die Menschen müssen aber wissen: von ihm aus entspringt Freude, Fröhlichkeit, Lachen und Scherz sowohl als Kummer, Unmut, Sorgen und Weinen. Durch das Gehirn nehmen wir wahr, begreifen, sehen und hören wir; es unterscheidet häßlich und schön, böse und gut, angenehm und widerwärtig. Ja, nach seiner Verfassung urteilen wir zu verschiedenen Zeiten verschieden. In ihm bilden sich Wutanfälle und Delirïen, Schreckbilder und Furcht bei Tag und Nacht, Träume, Illusionen und alle Gleichgewichtsstörungen unseres Bewußtseins.

Aber so lange das Gehirn nicht beunruhigt wird, ist der Mensch bei Verstand.“ [3])

Reinhold las und fragte mich erstaunt: „Wie, Du sagtest, wir seien ins Jahr 420 vor Christi Geburt hinaufgestiegen und hier soll schon jemand das alles geschrieben haben? Wo waren denn die experimentellen Beweise? Ist nicht, wie ich stets gehört habe, erst Franz Baco von Verulam der Schöpfer von Induktion und Experiment?“

„Bitte lies hier weiter,“ und ich händigte ihm einen zweiten Papyrus ein:

„Wenn man Wasser mit blauem Kupferocker oder mit Mennige verrührt, einem fast verdursteten Tiere — vorzüglich einem Schweine — einen großen Teil davon zu saufen gibt und ihm, während es säuft, die Kehle durchschneidet“ [4]) —

Hier unterbrach Reinhold seine Lektüre und blickte mich abermals groß an. In demselben Augenblick aber erschien Polybos, gefolgt von seinem Assistenten und streckte uns beide Hände zum Gruße entgegen: „Folgt mir in den Garten; es sprießen die Blumen, alles Leben keimt, heute sollen die Knaben sehen, was die Hennen seit gestern geleistet haben“. Damit führte er uns hinaus und da saßen drei Hühner, geschirmt von einem kleinen Schutzdach. Sein Gehilfe bückte sich und nahm jedem der erschreckten Hühner ein Ei weg, um die Beute in einem Tuche nach dem Hause zu tragen, der kleinen Werkstätte zu, die dem Operationszimmer angebaut war. Hier saßen drei Jünglinge von 16 bis 18 Jahren; sie erhoben sich, grüßten den hereintretenden Meister ehrerbietig und drängten sich nun um seinen Gehilfen, der die Eier aufbrach, um ihnen den Embryo des Hühnchens in drei verschiedenen Altersstufen vorzuführen.[5]) Reinhold erfaßte eine leichte Befangenheit. Er hatte ja auch einmal einen embryologischen Kurs mitgemacht. Wenn ihn aber Polybos jetzt gefragt hätte, ob er die Erklärung übernehmen wolle, so hätte er doch verbindlichst gedankt. Eine Keimscheibe und ein Hühnchen vom zweiten Tag hatte er ja auch einmal gesehen, spätere Stadien aber nur in mikrotomiertem Zustand kennen gelernt und Hühnchen der dritten Woche gar nie in Händen gehabt. Aber Polybos fragte ihn zum Glück nicht, sondern fuhr, auf die eifrigen Schüler hinweisend, fort: „Seht, daneben haben sie zum Ver-

gleiche keimende Pflanzen stehen. Denn die Wissenschaft von den auf der Erde wachsenden Pflanzen, so meine ich, entspricht dem Wissen der ärztlichen Kunst. Unsere Natur nämlich ist gleich dem Lande: die Sätze der Lehrenden sind gleich dem Samen; wer die Jugend schult, gleicht dem Säemann, der den Acker bestellt; der Ort, wo studiert wird, ist gleich der Nahrung, die aus der umgebenden Luft den Pflanzen geboten wird, die Arbeitslust ist gleich der Bestellung. All das aber bringt die Zeit zur Reife.“ [6])

Damit führte uns Polybos durch den Operationssaal, wo ein anderer seiner Assistenten von zwei Sklaven in den Zurüstungen für die Behandlung eines Armbruches unterstützt wurde. Zurechtgeschnittene Brettchen wurden gebracht, Binden bereit gelegt und wir sahen uns einen Augenblick in dem lichten Saale um. Zwei Operationstische nahmen die Mitte ein; an den Wänden Regale mit Salbenbüchsen, Arzneitöpfen, Schüsseln und Metallbecken. Der Sklave, welchem unsere Neugier auffiel, hob von einer in der Wand eingelassenen Marmorplatte ein Tuch weg und da lag ein ganzes spiegelblankes Instrumentarium. Dann wurde der Patient hereingebracht, und wir verließen den Saal, verabschiedeten uns von Polybos und sein Assistent geleitete uns durch die Stadt. Auf meinen Wunsch gingen wir über den Fischmarkt, den ich noch in keiner südlichen Hafenstadt ohne Genuß an der Formenfülle und Farbenpracht der Meeresbewohner besucht habe.

Auf drei breiten treppenartig zum Marktplatz aufsteigenden Längsreihen von Quadern hielten die Fischer ihre frische Beute aus Poseidons Reich feil. Unser Begleiter kaufte im Vorübergehen einen mächtigen Steinbutt sowie einen Korb voll kleiner Muscheln, und ließ beides nach dem Krankenhaus des Polybos schicken. Hierauf begann ich mit dem Assistenten eine längere Unterhaltung über die verschiedenen Arten von Fischen und Schaltieren, die er ebenso sicher mit Namen zu bezeichnen wußte wie wir, außerdem aber nannte er mir von jeder einzelnen Art die diätetische Verwendung, auf die der Meister den größten Wert lege.[7]) Reinhold trat etwas hinter uns zurück; er hätte sonst gestehen müssen, zwar eine Sepia von einem Polypen wohl unterscheiden zu können; aber Fische, nein, das war nie seine Spezialität gewesen.

„Ein elendes Pack übrigens diese Fischhändler“, murmelte unser Koër zwischen den Zähnen. „Archippos hat in der Tat nicht übertrieben. Und schon fühlt sich jeder Fischer heutzutage wie ein Feldherr. Nur noch die Köche sind ihnen darin über.“ Mit schalkhaftem Lachen hatte er dies eben noch gesagt; dann verabschiedete er sich um sich einem feierlichen Zuge anzuschließen, der die Stadt heraufkam und sich gegen das Asklepiosheiligtum bewegte: „Entschuldigt mich. Sie nehmen heute meinem Bruder Hippokrates den Eid ab. Ich sollte Zeuge sein“.[8])

Es schien mir, Reinhold habe nun genug gesehen und erlebt, um sich Gedanken auf ein Jahr hinaus zu machen. „Wir müssen mehr davon sehen“, meinte er aber halb neugierig, halb unruhig. „Hier leben Menschen, wie wir sie noch in der Jugend träumten, als uns die Sonne Homers noch schien, hier lebt die Forschung als freie Kunst, wie wir sie uns wohl dachten, als uns die Begeisterung für sie erfaßte und als wir beschlossen, uns ihr zu weihen. Wer ahnte damals, daß alles so ganz anders kommen würde?!“

„Beruhige Dich, mein lieber Freund, noch ist es früh am Tage, ein Sprung nach Athen und ein Ruck um hundert Jahre eine Kleinigkeit. Dort sollst Du nun gleich in vollem Glanz seines Ruhmes den erblicken, der für sechzehnhundert Jahre von der organischen Natur genug gesehen und gedacht hat, Aristoteles.“

Damit nahm ich Reinhold abermals bei der Hand. Während er mich treuherzig anschaute, war Kos verschwunden, und wir standen an den Pforten des Lykeions in Athen. Durch die Säulenhalle betraten wir den Garten, wo im Schatten der Baumalleen Gruppen lebhaft gestikulierender Männer und Jünglinge auf und abspazierten. Unbeachtet gelangten wir gerade dicht hinter Aristoteles selbst, der mit Menon eine Seitenallee aufgesucht hatte, um mit ihm über die Redaktion zoologischer Schriften zu konferieren.[9])

„Und nun weißt Du ja, Menon, ich will, daß auch jedes Einzelne an seinem natürlichen Ort sei und sich selbst gliedere, wie ein Organismus. In einer Wissenschaft, wo wir so ganz erst am Anfang stehen, dürfen wir aber darin nicht zu

weit gehen; wir erschweren sonst den Nachfolgern die Aufgabe, fortzufahren. Bedenke namentlich dabei, daß wir die Tiergeschichte an den Anfang stellen; sie soll dann zuerst einführen in die Tierwelt, wie sie uns in ihren einzelnen Erscheinungen entgegentritt und nach dem, was wir aus anderen Schriftstellern über sie erfahren. An zweite Stelle setzen wir, sobald die Schrift fertig ist, die „Teile der Tiere“, woraus jeder ersehen soll, welche Ursache einem jeden Organ innewohnt, an die dritte dann erst die Zeugungs- und Entwickelungsgeschichte. Denn es ist nur natürlich, daß man zuerst die Erscheinung, dann die Ursachen und zuletzt die Entstehung betrachtet. So erhalten wir das ganze Werk und wenn Du erst noch die nötigen Umstellungen vorgenommen hast, so diktiere ich dann die Einleitung.“ [10])

Die weiteren Worte gingen uns verloren, denn da wir am Ende der Allee angelangt waren, wagten wir es nicht, dem umkehrenden Meister unter die Augen zu treten. Mit einer Wendung nach links gewannen wir die neben der Allee entlang laufende Säulenhalle, wo wir uns unbemerkt unter andere Peripatetiker mischen konnten. Hier wurde die letzte Rede eines Isokratesschülers kritisiert, dort die Chancen der Wettkämpfer für den nächsten Fackellauf erwogen und damit wir nicht wie zwei traurige Marabus unter diesen temperamentvollen Menschen wanderten, sagte ich im Anschluß an das oben gehörte Gespräch zu Reinhold:

„Hast Du nun gehört, wie Bücher disponiert werden?“

„Das klang doch etwas sehr nach Schule,“ erwiderte er überlegen.

„Wohl, aber vergiß nicht, daß hier alles auf Schule und Wettkampf angelegt ist, und dann hast Du Dir doch gewiß einmal unsere Lehrbücher daraufhin angesehen, inwiefern ihre Gliederung der eines Organismus entspricht?.“

„Das könnte ich nicht behaupten, weder daß ich bisher darauf geachtet hätte, noch daß es so sei. Gott, wer schaut denn darauf! Wenn nur die einzelnen Tatsachen richtig sind und das Buch möglichst vollständig ist.“

„Nun ja, auch Aristoteles sagt, für den Naturforscher müsse die Kenntnis der Einzelheiten die Grundlage der Erklärung bilden.[11]) Aber meinst Du wirklich noch, ein Buch

bestehe lediglich aus so und so vielen petits faits, wie es aus Buchstaben und Wortbildern zusammengesetzt werde, auf die Art der Verbindung aber und auf die Struktur des Ganzen, die Entelechie, um mit dem Meister dort zu reden, komme nichts an? Die oberste Gliederung ist es vielmehr, die Geist und Geistlosigkeit, Bewußtheit und Unbewußtheit des Verfassers verrät. Achte nun einmal darauf, wenn Du Dir in Zukunft unsere Literatur besiehst."

Wir standen vor einem Raum, aus dem man durch eine Tür nach der Säulenhalle gelangte und der nach einem Garten hin sich öffnete. Da unterrichtete ein Schüler von Aristoteles, und er war ein trefflicher Zeichner. Eben hatte er ein Chamaeleon von der Größe eines Krokodils in den Sand skizziert und erklärte einigen Epheben die äußere Form der kleinen Kletterkünstler, die auf einem bereitgestellten Zweige herumturnten. Dann nahm er eines der Tierchen, ging zum Tisch, band es über ein Brettchen und hieß den beiseite sitzenden Vorleser aus einer Abschrift der Tiergeschichte vorlesen: „Das Chamaeleon hat im ganzen eine Körperbildung wie die Saurier. Die Rippen erstrecken sich abwärts und stoßen in der Unterleibsgegend miteinander zusammen, wie bei den Fischen und auf ähnliche Weise wie bei diesen erhebt sich der Rückgrat. Sein Gesicht ist dem des Schweinsaffen am ähnlichsten. Sein Schwanz ist langgestreckt und spitz auslaufend, auch läßt er sich in seinem größten Teil der Länge nach wie ein Riemen aufrollen. Es hat längere Beine als die Eidechse, so daß sich sein Leib höher über den Boden erhebt, doch sind die Bewegungen der Beine so, wie bei den Sauriern. Jeder Fuß ist in zwei Hälften geteilt, welche gegeneinander eine ähnliche Stellung haben, wie unser Daumen dem übrigen Teil der Hand entgegengestellt ist. Jeder dieser Teile ist bis auf eine kurze Strecke in einige Zehen gespalten, so daß an den vorderen Füßen drei nach innen und zwei nach außen liegen, an den hinteren dagegen zwei nach innen und drei nach außen. Sie haben Krallen ähnlich denen der Raubvögel. Sein ganzer Leib ist rauh wie der des Krokodils. Die Augen liegen in einer Höhle, sind sehr groß, rund und von einer ähnlichen Haut wie der ganze Körper bedeckt. In der Mitte ist zum Sehen ein kleiner Raum ausgespart, welchen es niemals mit der Haut bedeckt.

Es bewegt das Auge im Kreise und kann den Blick nach allen Richtungen wenden; so sieht es, was es will. Es verändert die Farbe, indem es sich aufbläht. Sie ist sowohl fast schwarz, wie die des Krokodils, als auch gelb nach Art der Saurier, beides scheckt sich pantherartig. Dieser Farbwechsel erstreckt sich über den ganzen Körper; daran nimmt auch gleichzeitig Auge und Schwanz teil. Es bewegt sich so träge wie die Schildkröten. Im Sterben wird es gelblich, und dieselbe Farbe besitzt es nach dem Tode. Die Lage der Speiseröhre und der Luftröhre ist dieselbe wie bei den Sauriern. Fleisch hat es nirgends außer kleinen Muskelmassen am Kopf und den Kinnladen, sowie an der Schwanzwurzel. Blut befindet sich nur im Herzen und um die Augen, sowie in der Gegend oberhalb des Herzens und in den von ihm ausgehenden Adern; aber auch in diesen nur auf eine ganz kurze Strecke. Das Gehirn liegt ein wenig oberhalb der Augen, steht aber mit ihnen in Zusammenhang. Nimmt man die äußere Haut von den Augen hinweg, so sieht man einen ringsumlaufenden durchschimmernden Teil daran, in Gestalt eines dünnen metallisch glänzenden Ringes. Fast durch den ganzen Körper erstrecken sich viele starke Häute, welche die der übrigen Organe weit übertreffen. Die Tätigkeit des Atmens dauert, auch wenn es ganz aufgeschnitten ist, noch geraume Zeit fort, während am Herzen sich noch schwache Bewegung bemerkbar macht, und es findet Zusammenziehung vorzugsweise in der Rippengegend aber auch an den übrigen Teilen des Leibes statt. Eine sichtbare Milz besitzt es nicht. Es hält einen Winterschlaf wie die Saurier.“[12])

Wir waren in der Türe stehen geblieben und hatten von weitem zugesehen wie unterdessen ein Chamaeleon zergliedert wurde. „Zoologischer Kurs,“ murmelte Reinhold. Nach dem, was er in der koïschen Schule gesehen hatte, war er nicht mehr so sehr überrascht. Aber die Zeichnung im Sande fesselte ihn; denn sie drückte mit voller Lebendigkeit im ganzen Körper des Tieres eine Bewegung aus, die mit wenigen Strichen alles besagte und Reinhold zu voller Anerkennung zwang. Wir traten etwas in die Halle hinein, um die Zeichen besser zu besehen. Da war denn auch die Wand mit Figuren aller Art bedeckt; insbesondere zunächst neben der Türe ein Riesenbild des Cephalopodenembryo mit dem charakteristi-

schen Dotter zwischen den Fangarmen, der Dotter war mit A bezeichnet, die Augen mit B und Γ.[13]) Und da standen denn auch noch in einem Gefäß mit Meerwasser die Eiertrauben von Loligo. Sie waren Reinhold deshalb eine besonders vertraute Erscheinung, weil einst sein Arbeitsnachbar an der zoologischen Station in Neapel sich speziell damit beschäftigt hatte, die Cephalopodenentwickelung an diesem Objekt zu studieren.

Wir traten in den Garten hinaus, dessen Anlage schon verriet, daß er weniger auf die Gesamtwirkung als auf einen besonderen Zweck berechnet sei. Es war die eigenste Schöpfung Theophrasts, der hier Beete nach Art der ägyptischen Pflanzengärten angelegt hatte, um gewisse Kräuter jederzeit zur Hand zu haben. Hecken von Lorbeer, Erdbeerbäumen, Erica arborea und düsteren Steineichen umgaben die ganze Anlage. In der Mitte aber, alles mit ihrer Krone majestätisch überschattend erhob sich die Riesenplatane. Ihre Wurzeln breiteten sich noch weiter aus als die Äste, wußte uns der arbeitende Sklave mit dienstfertiger Geschwätzigkeit zu erzählen. Denn als jüngst die Wasserleitung, die dem Rande des Gartens entlang läuft, nachgesehen wurde, da fanden sich noch Wurzelspitzen, dreißig Ellen weit vom Stamm entfernt. Der Meister Theophrast selbst habe es gemessen.

Der Sklave hätte uns gerne noch vieles erzählt; so oft ich aber die Hand hob, mußte er schweigen. Nur eines sollte Reinhold doch nicht entgehen. In den Beeten waren manche Pflanzen nach unseren Begriffen wirr durcheinandergesetzt; um so mehr fiel auf, daß doch wieder manche nach Familien zu Gruppen zusammengefaßt waren. Ich befragte darüber den Gartensklaven: „Man unterscheidet Kräuter, Stauden, Sträucher, Bäume“, sagt der Meister Theophrast; „der Baum aber ist das vollkommenste Gewächs, wie der Mensch das vollkommenste Tier“, sagt der Meister Theophrast; „der Baum besteht aus der größten Zahl von Geweben“, sagt der Meister Theophrast. Hier hob ich die Hand, um abzuschneiden. „Du verstehst mich nicht; was ich wissen will, ist: warum hier Lilien, Meerzwiebeln, Lauch beisammenstehen, dort Anis, Koriander, Dill, Kümmel und Fenchel.“

„Ach so; weil der Meister Theophrast sagt, sie gehören zu demselben γένος. „‚Genos‘, hörst Du, wandte ich mich

an Reinhold; Genos, das Gewordene, das Verwandte, der fundamentale Begriff für jede entwickelungsgeschichtliche Auffassung der organischen Natur. In dieser wunderbaren Sprache hat das sogar im Munde des Sklaven noch einen bedeutungsvollen Wohlklang und Sinn und ist nicht nur die Schachtel, darein so viel Spezies, als der Schöpfer am Anfang kreiert hat, gelegt werden." [14])

Fast hätte nun mein Freund Gelegenheit gefunden, eine Vorlesung zu hören. In den Wandelgängen des Lykeions pries man da und dort als Ereignis des Tages, daß ein neuer Sophist herübergekommen sei, aus Sizilien natürlich, er überbiete an Maßlosigkeit und Zungenfertigkeit alles Dagewesene. Ich wollte ihm diesen Genuß für den folgenden Tag aufheben und da es Mittag war und himmlisches Maiwetter ließ ich ihn bei Essen und Siesta sich ausruhen, wobei ich ihm noch einiges über die Prinzipien der aristotelischen Systeme der Biologie plaudernd einflößte und ihm dabei erklärte, daß längst vor Aristoteles bereits in der koïschen Schule ein zoologisches System existiert hatte.[15])

Die Sonne brannte nicht mehr so heiß und begann die Abhänge des Lykabettos sich in rot vergoldete und violett beschattete Flächen zu brechen, als wir uns abermals dem Lykeion zuwandten. Ich wollte ihm das protagoraeische Wort auslegen, daß das Maß aller Dinge der Mensch sei. Hatte doch kein geringerer als Goethe in diesem Wort die Grundbedingung der Naturforschung erkannt, wenn er sagte: „Wir mögen an der Natur beobachten, messen, rechnen, wägen, wie wir wollen; es ist doch nur unser Maß und Gewicht, wie der Mensch das Maß der Dinge ist." Und worin anders beruhte denn das tiefe Verständnis für die organische Natur als darin, daß eben der Blick der Griechen sich an den Formen des menschlichen Leibes geschult, die Übung, seines Anblicks sich zu freuen, auf alles Lebende übertragen hatte? Hätte doch meinem Freund nur verständlich sein können, wie folgerichtig sich die aristotelische Ansicht, daß die Form der Inbegriff des Wesens sei, aus der Kenntnis menschlicher Gestalt entsprungen war.[16])

Diesmal war es ein anderer Garten des Lykeions, den wir aufsuchten, die Palaestra.[17]) Einige Stufen abwärts

führten uns an den Rand der mit Sand bedeckten Palaestra und schon entstiegen einem anstoßenden Gemach zwei jugendliche Ringer von 15 Jahren, die olivenbraune Haut gesalbt mit Öl, um nach einigen Instruktionen des Pädotriben sich im Kampfe zu messen, während die sie begleitenden Pädagogen, zwei alte Sklaven, wovon der eine schielte und der andere einen hohen Rücken hatte, sich flüsternd in einer Ecke über ihre jungen Herren unterhielten.

Reinhold überflog eine leichte Schamröte, deren Ursache ich wohl begriff. Wo hätte er auch Gelegenheit gefunden, bei seiner dem Fortschritt der Bindegewebshistologie dienenden Wirksamkeit, einen Anblick wahrzunehmen, wie er jetzt sich ihm bot? Der Eindruck des Ungewohnten, die Befangenheit angesichts der menschlichen Schönheit in ihrer allernatürlichsten Form, brachten ihn etwas aus der Fassung.

Unterdessen hatten die beiden Ringer den Kampf schon begonnen. Der eine hatte sich dem anderen mit vorgebeugtem Körper genähert und war von ihm bereits zu Boden gedrückt, erhob sich aber mit Blitzesschnelle wieder, um den Gegner mit beiden Händen von der linken Seite zu fassen, während dieser rechts austretend, seinem Widerpart über den Rücken griff. So beharrten beide auf einige Augenblicke in ruhigem Gleichgewicht und boten das unübertreffliche Idealbild einer Ringergruppe dar, wie sie uns die Plastiker des Altertums veranschaulicht haben, nur durch das ihnen innewohnende Leben überaus viel schöner und ausdrucksvoller als das schönste Kunstwerk. Aber nicht nur der Typus des Menschen trat in glänzendster Wirklichkeit meinem Freunde vor Augen. Ich selbst wurde erst gewahr, wie richtig Aristoteles urteilte, wenn er die verschiedenen Schönheitstypen als gleichberechtigt anerkannt wissen wollte, da die beiden Kämpfer in ihrem Körperbau jeder auf seine Weise vollkommen waren. Und wie fein war seine Beobachtung gewesen, daß Schenkel und Wade in umgekehrten Korrelation ausgebildet seien.[18]) Der Kampf entschied sich, begann aber zwischen einem neu antretenden Paar in ähnlicher Art sogleich wieder.

Während dieser gymnastischen Übungen schien es mir, als ob meinem Freund eine neue Welt aufgehe und als ob er zu ahnen beginne, daß Naturforscher, die täglich ihr Auge

so am Menschen weideten, auch die übrigen Organismen mit anderen Augen ansehen müßten. Aber ich wollte seine innere Arbeit an sich selbst nicht unterbrechen. Unterdessen hatte sich vom Hauptgebäude des Lykeions her eine Gruppe von Peripatetikern angesammelt, die mit beinahe lebhafterer Teilnahme, als mein Freund, der alles zum ersten Male sah, das Schauspiel genossen, das ihnen doch ein alltägliches sein mußte. Ihnen aber konnte es tausendmal mehr besagen, als uns Hyperboräern und Barbaren. Die Gewöhnung an das Empfinden des Formenschönsten und Lebendigsten, die Konzentration ihres Vorstellungskreises um das agonale Leben, worin sie von frühester Jugend an aufgewachsen waren, und die Hoffnungen für ihre Kultur beim Anblick des neu heranwachsenden Geschlechts — all das erzeugte das natürlichste Hochgefühl, eine Intensität der Empfindung für alles Leben, die wir ebenso reich mitzuempfinden zu stumpf sein mußten.

Bei sinkender Sonne erschien der Gymnasiarch und ließ den Ringkampf einstellen, da es Zeit sei, das Gymnasium zu schließen. Die Kämpfer ordneten sich zum Heimgehen und in ihren verschiedenen Stellungen erinnerten sie meinen Freund an die schönsten Bildwerke klassischer Kunst. Stand dort nicht der Apoxyomenos? Dort Antinous? Dort Harmodios und Aristogeiton? Und Reinhold verstand, warum in Neapel, als er einmal seine müden Augen ausruhen wollte und eines Sonntags die antiken Skulpturen des Museums besah, sie ihm so fremdartig vorgekommen waren; er hatte die Vorbilder dafür nie gesehen, jedenfalls nie bewußt, nie im Zusammenhang mit Vorstellungen von der Plastik der gesamten organischen Natur.

Ich überließ ihn gerne seiner Reue. War ich doch davon überzeugt, sie werde ihn zu der Erkenntnis zurückführen, daß ein Naturforscher allerdings heute an irgend einer Stelle seiner Wissenschaft sich gründlich zu vertiefen habe, daß er aber dabei seinem Empfinden für die Natur, der Aufnahme beständig neuer Sinneseindrücke ihres wechselvollen Kampfspiels keine Schranken setzen dürfe, wenn ihn jene Vertiefung nicht nach dem Gesetz der Trägheit hinabziehen soll. Mein Freund war in diese Gefahr geraten; noch konnte ich hoffen, daß er lebensfrisch genug sei, das Gleichgewicht in sich her-

zustellen, das allein eine weitere menschlich und kulturell wertvolle Entwickelung des Forschers verbürgt. Wo und wie anders hätte er stärkere und glücklichere Anregungen empfangen können, seiner alten Begeisterung, die unter Sorgen verstaubt war, zu neuem Leben zu verhelfen, als wenn er sah, wie hier in Griechenland unsere Wissenschaft der Fülle des Lebens selbst entquoll? Die Knaben hatten das Gymnasium verlassen, gefolgt von ihren Pädagogen, und schon wandten sich auch die Peripatetiker heimwärts zum gemeinsamen Symposion und verschwanden in den Baumalleen. „Es ist Zeit, daß auch wir gehen," sagte ich zu Reinhold, „laß uns vor unserer Rückkehr nur noch einen kurzen Aufenthalt in Alexandrien nehmen, siebenzig Jahre später.

Wir standen am frühen Morgen in einem Säulengange des anatomischen Instituts. Das verriet schon der charakteristische Leichengeruch, der auch im reinlichsten Gebäude dieser Art unvermeidlich ist. Allerhand Gerätschaften zur Suspension der Leichen, einige Seziertische und ein prunkvolles Katheder schmückten den in reizenden Proportionen gehaltenen Rundbau, der als Seziersaal diente und nach dem Garten hin lag. Alles prangte im reinsten Marmor mit Gold verziert. Durch eine zierliche rings die Mauer krönende Kolonnade strömten die schimmernden Lichtmassen herab, und man hätte beim Betreten der wenigen Stufen eher geglaubt, in das Badehaus eines Fürsten hinabzusteigen, als in einen der ernsten Wissenschaft gewidmeten Raum. Am meisten aber erregte unsere Neugier ein eigentlicher mit allem Prunk ausgestatteter Thron, der dem Katheder gegenüber angebracht war. Da pflegte Ptolemäos Philadelphos Platz zu nehmen, wenn er den Sektionen beiwohnte.

Es war eine sonderbare Szene gewesen, die sich am Vorabend in den Gemächern des Königs abgespielt hatte. Der Finanzminister kränkelte seit längerer Zeit. Alle, auch noch so kostbaren Arzneimittel waren erfolglos verwendet worden. Der König wollte und durfte ihn nicht verlieren; er besprach daher mit Herophilus die Chancen einer Operation. Herophilus aber benützte den Anlaß, um dem König einen längst gehegten Wunsch auszusprechen; war das doch der Moment, wo der König der Ärzte über dem aller

Völker Ägyptens stand; die Gelegenheit durfte nicht unbenutzt vorbeigehen. Zögernd nur hatte Herophilus gestanden, die notwendige Vorbedingung für einen chirurgischen Eingriff sei das Experiment am Lebenden. Dabei könnte man, abgesehen von dem eigentlichen Zweck eine Reihe von Angaben des großen Hippokrates prüfen, die anders nicht zu entscheiden seien. Philadelphos aber zauderte nicht lange, und auf ein paar Piraten kam es ihm nicht an, galt es doch den Minister zu retten. Während wir noch an der Türe des Seziersaales standen, kamen Sklaven, Bretter mit frisch geschliffenen Messern tragend; dann wurden Gefäße aller Art hereingebracht und am Fuße des Thrones ein Weihrauchbecken aufgestellt. Wir schlichen uns längs der Mauer ein, um unbemerkt der Sektion zuzusehen, allerdings ohne so recht zu ahnen, was kommen würde.

Als alles bereit war, trat Herophilus mit einem kleinen Gefolge von Assistenten und Dienern herein, blickte etwas nervös umher und ließ dann seinen Blick flüchtig auf uns haften. Er schien einen Moment zu glauben, wir seien die beiden ihm verfallenen Schächer; denn ein sarkastisches Lächeln umspielte seinen Mund, als er seinen Irrtum bemerkte, während er fortfuhr, das Lokal zu mustern. Er gab dann einem Assistenten leise Befehle und während alles sich im Kreise ordnete, bestieg der Vorleser das Katheder, um nach Gewohnheit die von ihm vorzulesenden Rollen der hippokratischen Schriftensammlung bereitzulegen.

„Heute liesest Du nur, wenn ich frage,“ bemerkte Herophilus, „es gibt keine gewöhnliche Anatomie.“

Der König erschien, gefolgt von zwei Edlen und zwei Pagen; alles warf sich auf die Erde nieder, und kaum hatte Ptolemäus seinen Thronsessel bestiegen, so brachten drei Schergen das Opfer der bevorstehenden Vivisektion. Der Anblick des geknebelten Seeräubers hätte einen für das Leben der Anwesenden zittern machen können, hätten nicht die schweren Fesseln durch ihr Klirren die Zuschauer beruhigt. Der trotzige Kopf voll kurzer Locken, das wuchtige Profil, der Stiernacken und die athletische Muskulatur ließen keinen Zweifel darüber, welchem der Anwesenden die Natur selbst die Herrscherwürde zugesprochen hätte. Ares schien in eigener

Person dazustehen. Der forschende Geist des Gelehrten hatte aber über die weltliche Macht des Königs gesiegt, der Purpur wiederum über die menschliche Bestie vollendetsten Schlages, die Psyche über die Physis. So war der Kampf bereits entschieden, und der Pirat lag, rasch von der Übermacht auf den Marmortisch geworfen, gefesselt vor den Augen des Königs. Ob er wohl den Schmerz empfinden würde, wie wir? Reinhold erinnerte sich jener russischen Bauern, die auf die heftigsten Züchtigungen kaum reagierten, und die trotzige Gefaßtheit des Opfers ließ hier dasselbe erwarten. Außerdem hatte ihm Herophilus zugleich mit einer opulenten Mahlzeit eine große Dosis Mohnsaft reichen lassen, um der Dämpfung des Bewußtseins nachzuhelfen.[19])

Auf einen Wink des Königs begann der Vivisektor sein Werk. Der Längsschnitt der Linea alba entlang bis zum Brustbein war im Nu angelegt. Ares knirschte fürchterlich mit seinen diamantenen Zähnen, leises Stöhnen entrang sich seinen Lippen. Herophilus ließ die eröffnete Bauchwand auseinanderhalten, um die Peristaltik der Eingeweide zu beobachten und die Art ihrer Bewegung begierig zu verfolgen. Was er erwartet hatte, war eingetroffen: Die Chylusgefäße hatten sich infolge der genossenen Mahlzeit angefüllt und er sah sie in die drüsenartigen Körper eintreten, ganz so, wie er es unter denselben Verhältnissen einst bei Tieren beobachtet hatte. Er legte beidseitig Querschnitte an und ließ durch Schiefstellung des Tisches die Eingeweide nach rechts prolabieren, sodaß der Zwölffingerdarm, auf dessen Entdeckung er nicht wenig stolz war, sichtbar wurde. Schon ließ sich der Arterienpuls mit voller Deutlichkeit beobachten, und die unter das warme Zwerchfell gelegte Hand erschütterten die Schläge des Herzens. Aber noch suchte der Anatom den Sitz des Blutzentrums in der Leber, und sah er auch die Arterien pulsieren, so konnte es doch nur der Lebensgeist, das Pneuma sein, was sie bewegte. Noch ehe er die bluttriefende und dampfende Hand zurückzog, wurden Ares die Augen verbunden und die verschiedenen aus dem unterdrückten Gewinsel heraus gegebenen Antworten verrieten dem tastenden Anatomen die Empfindlichkeitsunterschiede der verschiedenen berührten Stellen und die Qualitäten des Schmerzes. Was Herophilus nie in solcher

Mannigfaltigkeit zu unterscheiden vermocht hatte, das waren die Grade der Härte und Weichheit bei diesem und jenem Organ; jetzt auch erst sah er zum ersten Male die richtige Färbung der normalen Gewebe des lebenden Menschen. Rasch suchte er die Stelle sich einzuprägen, an der er den lebensgefährlichen chirurgischen Eingriff an seinem hohen Patienten wagen sollte; dann nickte er mit dem Kopfe und mit einem sichern Schnitt eröffnete er das Zwerchfell, um die tötliche Wirkung dieses Schnittes darzutun, da durch das Eindringen der Luft in die Pleurahöhle die Respiration stillgestellt wurde. In demselben Augenblick bäumte sich die vorher schon krampfhaft spielende Muskulatur noch einmal auf. Mit hellem Klang war unter der Gewalt des rechten Oberarms ein Glied der Eisenkette zersprungen. Ares hatte ausgerungen.

Herophilus richtete sich jählings auf, um Atem zu schöpfen. War es die physische Anstrengung, die ihn ermüdet hatte, oder eine Vorahnung, daß kommende Geschlechter ihn als den Würger brandmarken würden? Sinnend rekapitulierte er all die Eindrücke, die er mit Auge und Hand wahrgenommen hatte, durch die seine persönliche Erfahrung so unermeßlich bereichert worden war und die ihn in seiner verantwortungsvollen Aufgabe leiten sollten. Er überließ es seinen Gehilfen, die weitere Anatomie zu vollenden. Nur einmal noch legte er Hand an. Er hatte die Schädelhöhle eröffnen lassen und entnahm ihr gewandt das Gehirn, um es, abseits gewendet, in seiner eigenen Weise zu zerlegen, so daß die Chorioidealhäute sichtbar wurden.[20])

Reinhold war von dem Anblick dessen, was er eben hinter sich hatte, aufs Innerste ergriffen. Fast automatenhaft verließ er den Saal. „Das ertragen unsere Nerven nicht mehr," raunte er mir zu, als der zweite Pirat desselben Weges an uns vorbeizog, den der erste gekommen war. Nein, mit diesem Eindruck konnte ich ihn nicht von Alexandrien scheiden lassen, nicht aus dieser Folterkammer ihn ins volle Bewußtsein zurückrufen. Ich brachte ihn also in den königlichen Garten, wo die ausgesuchtesten Pflanzen, die seltensten Tiere der ostafrikanischen Küste, Libyens, Persiens und Arabiens vereinigt waren. Ein Gang durch das Serapeion und das Museion sollte ihm von dem Reichtum antiken Wissens, das hier in tausen-

den und abertausenden Rollen niedergelegt war, einen Begriff geben. Was ich ihm jedoch nicht mehr verschaffen konnte, das war der Einblick in eine philosophische Schule vom Range der koïschen und der peripatetischen. Wohl existierten noch Peripatetiker, aber der empirische Boden des Meisters war ihnen längst unter den Füssen entschwunden.

Es schien mir hohe Zeit, meinen Freund ins Leben zurückzuführen, um von ihm Abschied zu nehmen. So brachte ich ihn denn auf den Stuhl in seinem Laboratorium zurück, nahm seine Hand und rief: „Reinhold!“ Er schlug die Augen auf und starrte verwundert in die Ferne, als wollte er sich vergegenwärtigen, was mit ihm geschehen sei. Mir selbst war der Mechanismus des Rätsels Nebensache, war es mir doch gelungen, ihm das innere Auge dafür zu öffnen, daß die Zeit, der wir angehören, uns nur einen unvollkommenen Querschnitt der Wissenschaft veranschaulicht. Wollen wir aber die Wissenschaft als Organismus erfassen und begreifen, so genügt die Kenntnis dieses Querschnittes nicht, auch wenn wir sein äußerstes Detail erspüren; wir müssen tiefer gehen, müssen die Entwickelungsgeschichte der Erkenntnis soweit wie möglich an der Wurzel erfassen, wo sie eben aus dem Keim menschlichen Bewußtwerdens nach freier Entfaltung strebt. Nur so wird sie zu einer wirklich aktiven Potenz in unserm Dasein und in dem der Gesellschaft und befähigt uns, neues und organisches wissenschaftliches Leben in denjenigen zum Durchbruch bringen zu helfen, die unserer Fürsorge anvertraut sind.

„Was war das,“ begann Reinhold zu fragen, als ich mich erhob, „bleibe da und erkläre mir —“

„Lieber Freund, ich muß fort, der Zug verläßt die Stadt in einer Viertelstunde. Für heute laß Dir nur das eine gesagt sein: Historia vitae magistra! Auf Wiedersehen, wenn Du mich im nächsten Frühjahr in der alten Humanistenstadt am Rheine aufsuchen wirst.“

Anmerkungen.

Man wird verstehen, warum ich mich durch die paraenetische Absicht meines Vortrags zur erzählenden Darstellungsform entschlossen gesehen habe. Eine systematische Behandlung des Stoffes verbot sich ebensowohl durch die Ausdehnung des Materials, wie durch den Mangel an geeigneten Vorarbeiten über Geschichte der antiken Biologie. Unter diesen Umständen konnte ich aber die Zitate, obschon sie vielfach den besten Übersetzungen entstammen, nicht wörtlich wiedergeben; auch musste ich Autoren redend auftreten lassen, ohne daß der Wortlaut mehr als den in ihren Schriften ausgedrückten Gedanken oder den von ihnen überlieferten Entdeckungen entsprechen konnte. Ich verzichte somit von vornherein darauf, Ansprüchen an philologische Genauigkeit genügen zu wollen. Ebensosehr bedarf es eines Wortes der Aufklärung gegenüber biologischen Fachgenossen. Um Mißverständnissen vorzubeugen, versichere ich ausdrücklich, daß es mir durchaus fern liegt, die Hilfsmittel der modernen Technik, deren ich mich bekanntlich in zahlreichen Spezialuntersuchungen auch bedient habe, herabzusetzen oder sie der Geringschätzung Unbeteiligter preiszugeben. Statt des Mikroskopikers hätte ebenso gut ein anderer Spezialist, der den Zusammenhang seiner Spezialität mit der Gesamtheit der biologischen Disziplinen verloren hat, zum Vorwurf genommen werden können. Man wird mir aber nicht bestreiten wollen, daß ein tragischer Konflikt — und zwar nicht nur innerhalb unserer Wissenschaft — sich allzuleicht herausbildet, wo eine Spezialität, besonders wenn sie von großem technischen Hilfsmittel abhängig ist, den ihr Ergebenen so völlig absorbiert, daß er nicht mehr Herr der Sache bleibt, sondern, von ihr beherrscht, einer pessimistischen Auffassung der Wissenschaft überhaupt zum Opfer fällt. Gegenüber dieser Verzichtleistung auf individuelle Werte im wissenschaftlichen Leben scheint mir das wirkungsvollste Gegengewicht in der Beschäftigung mit der Geschichte der eigenen Wissenschaft gegeben, zu dem der Forscher in anderen, philosophischen, historischen, juristischen und theologischen Fächern eo ipso mehr genötigt ist, als er es in unseren Disziplinen zu sein scheint. Gerade dem Biologen aber, der unter dem Eindruck der Entwickelungslehre steht, sollte zu begreifen nicht schwer fallen, daß auch der Organismus der Wissenschaft eine Entwickelungsgeschichte hat, die noch niemals studiert worden ist, ohne daß für den Fortschritt der Wissenschaft selbst neue Anregungen daraus entsprungen wären.

[1]) L. Roß, Reisen nach Kos, Halikarnassos, Rhodos und der Insel Cypern, Halle 1852. — Rud. Herzog, Vorläufiger Bericht über die archäologische Expedition auf der Insel Kos im Jahre 1902 und Von der Kos'schen Expedition. Mittlg. z. Gesch. der Med. u. Naturw. 1903.

[2]) Der hier wiedergegebene Ausspruch entstammt den „Vorschriften" der hippokratischen Sammlung. Ich zitiere ihn, wie die weiteren hippokratischen Texte nach der Übersetzung von R. Fuchs, München 1895, da sie leichter

zugänglich ist als die großen Originaltexte, bemerke aber von vornherein, daß ich die Fuchssche Übersetzung jedesmal nur frei mutatis mutandis wiedergebe. Ich lasse sie nun aber in diesem Falle auch wörtlich folgen, um mich dem gegen mein eigenes Vorgehen gerichteten Vorwurf des Hippokratikers nicht zu entziehen: I p. 64/65 Kap. XII: „Wenn man um der Menge willen eine öffentliche Vorlesung veranstalten will, so ist das kein sehr rühmliches Verlangen, wenigstens hüte man sich, poetische Zeugnisse zu verwenden, denn das würde ein Unvermögen in dem Mühe-aufwande verraten. Ich verwerfe nämlich, soweit die Praxis in Betracht kommt . . . . .“

[3]) Hippokrates, „Die heilige Krankheit“ (Fuchs, Bd. II p. 554 u. p. 561, 562 Kap. VIII u. Kap. XVII).

[4]) Hippokrates, „Das Herz“ (Fuchs, Bd. I p. 147 Kap. II). Da es an dieser Stelle nur darauf ankommt, zu zeigen, daß bereits die Hippokratiker Experimente beschrieben und daher wohl auch veranstaltet haben, habe ich die Fortsetzung, nämlich den Schluß, der aus dem Experiment gezogen wird, weggelassen, weil er infolge ungenauer Beobachtung falsch ist. Schon im Altertum wurde er als irrtümlich erkannt, wie die ausführliche Polemik von Aristoteles dagegen zeigt (de partib. anim. 665a). Auch Galens Anschauungen über den Bau des Nervensystems beruhen auf mannigfach angeordneten Experimenten (vgl. hierzu u. a.: F. Falk, „Die geschichtl. Entwicklung der experim. Medicin“. Virchows Archiv Bd. 132. 1893).

[5]) Die hippokratische Schrift „Die Entstehung des Kindes“ (Nr. 15b der Fuchsschen Übers. Bd. I p. 217 u. ff.) ist ein glänzender Versuch, die Analogie in der Entwickelung von Pflanze, Tier und Mensch durchzuführen. Kap. XVIII enthält die Anleitung zum Studium der Entwickelung des Hühnchens im bebrüteten Ei. Vgl. hierzu B. Bloch, „Nova Acta Acad. Leop.-Carol.“ 1904.

[6]) Der einleitende Satz ist aus der in Anm. 5 erwähnten Verallgemeinerung der Einheit der organischen Entwickelung zu begründen. Das Übrige ist Kap. III der Schrift „Das Gesetz“ (Fuchs, Bd. I p. 4).

[7]) In der hippokratischen Schrift „Die Diät“ schildert ein koïscher Arzt die verschiedenen Nahrungsmittel, worunter in Kap. XII die Wassertiere, wie sie auf südlichen Fischmärkten noch heute feilgeboten werden, unter Angabe ihres Nährwertes im Einzelnen. Vgl. hierzu meine in Anmerkung 15 erwähnte Schrift.

[8]) „Der Eid“, Fuchs, Bd I p. 1.

[9]) Über die peripatetische Schule und die Art des Unterrichts in ihr vgl. E. Zeller, „Die Philosophie der Griechen“, 3. Aufl., II 2. Übrigens ist der „Ruck um hundert Jahre“ nicht wörtlich zu nehmen. Die Episode zu Kos wäre wohl etwas später als 420 anzusetzen, die im Lykeion dagegen etwa ins Jahr 324, da Aristoteles 323 Athen verließ (Zeller l. c. p. 36 Anmerkung 1).

[10]) Die hier geschilderte Szene beruht auf folgenden Quellen: Menon, der Schüler von Aristoteles, ist als Redaktor der Schriften des Meisters durch Entdeckung des Londoner Papyrus 137 und die anschliessende Literatur in

den Vordergrund getreten. Der Inhalt der Besprechung entspricht der Auffassung von der Disposition der zoologischen Schriften des Aristoteles, welche Titze und v. Frantzius (Arist. vier Bücher über die Teile der Tiere, griech. u. deutsch, Leipzig, 1853) mit Erfolg vertreten haben. Man vergleiche besonders das I. Buch der Schrift über die Teile der Tiere.

[11]) Aristoteles, Tiergesch. (herausgeg. u. übers. von Aubert und Wimmer, 1868) I. 36.

[12]) Aristoteles, Tiergesch. II 41—44. Hierbei ist zu bemerken, daß sich bei der Übersetzung von Aubert und Wimmer eine sinnlose Wiedergabe der Stelle τραχὺ δ'ἔχει ὅλον τὸ σῶμα eingeschlichen hat: „Sein ganzer Leib ist auch (statt: rauh) wie der des Krokodils".

[13]) In der Tiergeschichte weist der Text zweimal auf Zeichnungen hin, die ihn begleitet haben und deren Teile wie unsere heutigen Figuren Buchstabenbezeichnungen getragen haben müssen. Die eine dieser Figuren veranschaulichte die männlichen Zeugungsorgane (Tiergesch. III 9), die andere den Embryo der Cephalopoden (ebenda V 89). Auch I 86 verweist Aristoteles auf die Diagramme in den Anatomien.

[14]) Theophrast von Eresos, der Schüler und spätere Nachfolger des Aristoteles als Haupt der peripatetischen Schule, war ca. 12 bis 16 Jahre jünger. Es widerspricht nichts der Annahme, daß er schon damals, unmittelbar bevor Aristoteles Athen verließ, eine gewisse selbständige Lehrtätigkeit an der Seite des Meisters ausübte. Daß er im Komplex des Lykeions einen Garten besessen habe, ist nicht bekannt; man wird mir aber diese Fiktion verzeihen in Anbetracht dessen, daß ich einige Hauptsätze seiner Botanik einführen wollte, daß ferner Pflanzengärten schon vorher in Ägypten existierten und daß endlich keine positiven Angaben dieser Annahme widersprechen. Die Platane des Lykeions ist in der Naturgeschichte der Gewächse (Übers. von K. Sprengel, 1822) erwähnt I, 7, 1; die Unterscheidung der Pflanzen nach dem Habitus I, 3, 1; die Vollkommenheit des Baumes I, 1, 12; die Gewebe (gleichartigen Teile) I, 2, 1.

Soweit ich die botanisch-historische Literatur kenne, ist darauf nicht geachtet worden, daß Theophrast in der Aufzählung einzelner Pflanzen nicht regellos verfährt, sondern mehrfach solche aneinanderreiht, die auch wir noch zu denselben Familien zählen (z. B. Gramineen, I, 6, 5, Nadelhölzer, I, 12, 1, Umbelliferen, I, 11, 2, Liliaceen, I, 6, 7, Rosaceen, II, 7, 8). Es sind diejenigen Formenkreise, aus denen auch durch die Patres botanici eine gewisse Verwandtschaft herausgefühlt worden sein muss. Theophrast bezeichnet zwar nicht gerade diese Formenkreise ausdrücklich als Gattungen, aber engere, z. B. die Eichen, deren einzelne Arten er unterscheidet (III, 8, 1 und IV, 1, 1). Insofern glaubte ich mich berechtigt, diesen Begriff auf jene Formengruppen übertragen zu dürfen, um so mehr, da er ja auch viel reichlicher für die Tierwelt von Aristoteles verwendet wird und außerdem bei Theophrast prinzipiell ebenso (I, 2,4), auch für die gesamte Pflanzenwelt (I, 2, 3) gebraucht wird, es außerdem an dieser Stelle nur auf die Bedeutung der klassischen Ausdrucksform für einen Formenkreis von organischen Individuen ankam.

[15]) Aristoteles kann nicht mehr als der eigentliche Schöpfer des ersten zoologischen Systems betrachtet werden. Ein solches muß vielmehr schon in der koïschen Schule existiert haben. Die aristotelische Systematik hat einen langen Entwickelungsgang hinter sich, dessen dunkle Spuren sich verfolgen lassen. Das Verdienst jedoch, anatomische Einteilungsgründe der Systematik zuerst zu Grunde gelegt und danach die größte Heerschau über die Tierwelt organisiert zu haben, bleibt ihm unter allen Umständen. Vgl. meine Schrift „Das koïsche Tiersystem, eine Vorstufe der zoologischen Systematik des Aristoteles". Verh. Naturf. Ges. Basel, Bd. XV 3, 1904.

[16]) Zeller II. 2 p. 479 u. ff.

[17]) Über Gymnastik vgl. die bei R. Fuchs („Gesch. d. Heilkunde b. d. Griechen, Handbuch d. Gesch. d. Medizin", 2 Lfg. p. 187 Jena 1901) aufgeführte Literatur. Außerdem J. L. Ussing, Darstellung des Erziehungs- und Unterrichtswesens bei den Griechen. Übers. Altona 1870. J. B. Egger, Begriff der Gymnastik bei den alten Philosophen und Medizinern. Sarnen 1903.

[18]) Bis zu welcher Feinheit die Proportionenlehre des menschlichen Körpers ausgebildet war und wie sie für Aristoteles der Ausgangspunkt zur Beurteilung der tierischen Proportionen wurde, geht aus zahlreichen Stellen seiner zoologischen Schriften hervor. Die hier speziell aufgeführte Beobachtung stammt aus der Tiergeschichte (I. 59), bedurfte aber für den Vortrag einer leichten Modifikation. Man vergleiche außerdem: I 57, II 25, de partib. IV 9.

[19]) Die Wirkung des Opiums war schon den Alten bekannt.

[20]) Diese Schilderung einer Vivisektion setzt sich zusammen aus Beobachtungen, die tatsächlich auf Herophilos' anatomische Studien zurückgehen (Chylusgefäße, Puls, Plexus chorioidei des Gehirns etc.) und andernteils aus den Angaben von Tertullian (de anima 10) und Celsus, dessen Angaben über Hergang und Zweck einer Vivisektion ich in der Darstellung möglichst gefolgt bin. A. Corn. Celsi de medicina libri octo, ed. Daremberg. Lipsiae 1859. Prooem. p. 4, 36 ff u. p. 7, 27 ff. An diese Angaben hat sich eine umfangreiche Literatur der moralischen Entrüstung angeschlossen, die teilweise aus gelegentlichen Urteilen besteht, zu denen sich beinahe jeder Autor, der mit dieser Stelle in Berührung gekommen ist, veranlaßt fühlte; anderseits aber bemüht sie sich, den wirklichen Sachverhalt zu eruieren, z. B. Fuchs im Rhein, Mus. N. F. 52 p. 382. Die vorgebrachten Gründe konnten mich jedoch nicht davon überzeugen, daß die Schilderung des Celsus eine Erfindung sei. Wenn man bedenkt, welchen Foltern die ersten Christen ausgeliefert waren, mag auch immerhin mancher Bericht auf Übertreibung beruhen, so erscheint daneben eine rasch und planmäßig durchgeführte Vivisektion beinahe als eine Gnade.

5

Das koische Tiersystem,

eine Vorstufe der zoologischen Systematik des Aristoteles.

Von

Rudolf Burckhardt.

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Rudolf Burckhardt.

I. Einleitung.

Die aristotelische Tiersystematik hat bisher als der erste Versuch gegolten, die Mannigfaltigkeit der tierischen Organismen nach logischen Prinzipien zu ordnen. „Von den Versuchen Früherer, das Tierreich einzuteilen, sagt *J. V. Carus* (No. 5 des Litteraturverzeichnisses, p. 77), ist höchstens mit Ausnahme einiger Ausdrücke, kein Zeichen auf die Nachwelt gekommen“ und nachdem er eine Übersicht des aristotelischen Systems gegeben hat (p. 84): „unverkennbar liegen in dem hier flüchtig skizzierten Systeme die Keime zur Entwicklung der natürlichen Anordnung des Tierreichs.“ Dies ist die in der Zoologiegeschichte heute giltige Anschauung und sie mag durch einen weiteren Ausspruch von *Carus* illustriert werden, der lautet (p. 62): „Der Hippokratiker hier zu gedenken, könnte natürlich erscheinen, da ja die menschliche Anatomie ihnen besonders nahe lag. Der ganze Gewinn, welchen Zootomie und vergleichende Anatomie dieser Schule verdankt, ist aber keineswegs nennenswert.“ Auch der von einem vorzüglichen Kenner und Übersetzer der hippokratischen Schriften, von *R. Fuchs* verfasste Abschnitt des neuen Handbuchs der Geschichte

der Medizin berichtet nichts über Verdienste der hippokratischen Schriftsteller um die Zoologie und deren Systematik. Beim Studium des Corpus hippocraticum fiel mir nun auf, dass der Verfasser des zweiten Buches der Schrift *περὶ διαίτης* zweiundfünfzig Tiere mit Namen aufzählt und deren diätetischen Wert bespricht. Die ganze Aufzählung, die sich zwischen eine solche der vegetabilischen Nahrungsmittel (Kap. II—IX nach Fuchs, No. 8) und der animalischen Produkte (Kap. XIV und XV) einschiebt, umfasst nur vier Kapitel (X—XIII). Das letzte derselben fällt für unsern vorliegenden Zweck erst in zweiter Linie in Betracht, da es von den Beziehungen zwischen physiologischen und anatomischen Eigenschaften der Tiere und deren entsprechendem Nährwert für den Menschen handelt. Die drei übrigen aber enthalten eine Reihenfolge von Tieren mit Rücksicht auf ihren Nährwert, die ich eines eingehenderen Studiums und namentlich einer sorgfältigen Vergleichung mit dem aristotelischen Tiersystem für bedürftig halte, da den betreffenden Kapiteln von den Hippokratesforschern wohl in manchen Punkten, aber nicht mit Rücksicht auf die zoologische Systematik, insbesondere nicht auf die zeitlich zunächst liegende und ausführlichste des Aristoteles, Aufmerksamkeit geschenkt worden ist.

II. Vorbemerkung über die grossen Gruppen der aristotelischen Tiersystematik.

Das aristotelische System der Tiere hat durch *J. B. Meyer* (No. 14) eine eingehende Darstellung erfahren, die auch in weitestem Umfange bestätigt und acceptiert worden ist. Auf Grund derselben, sowie im Anschluss an ihre eigenen Untersuchungen haben *Aubert* und

Wimmer die grossen Gruppen des aristotelischen Tiersystems zu einem Gesamtbilde vereinigt (No. 1, p. 60), da bei *Aristoteles* selbst eine zusammenhängende Darstellung der obersten Gliederung der von ihnen geschilderten Tierwelt fehlt. Diese Übersicht leidet nur daran, dass in ihr die *ἔντομα*, die die Autoren an dritter Stelle unter den Wirbellosen folgen lassen, von Aristoteles erst als vierte Gruppe unter den *ἄναιμα* besprochen werden, und zwar wiederholt, besonders aber an der Hauptstelle (No. 1, IV 1 und 68—77), wo die Blutlosen eingehend in ihren Hauptmerkmalen charakterisiert werden. Mit dieser Korrektur würden also die obersten Glieder des aristotelischen Systems lauten:

I. *ἔναιμα* (Bluttiere).
  1. *ζῳοτοκοῦντα ἐν αὐτοῖς* (Säugetiere).
  2. *ὄρνιθες* (Vögel).
  3. *τετράποδα ἢ ἄποδα ᾠοτοκοῦντα* (Reptilien).
  4. *ἰχθύες* (Fische).

II. *ἄναιμα* (Blutlose).
  1. *μαλάκια* (Weichtiere).
  2. *μαλακόστρακα* (Weichschaltiere, Crustaceen).
  3. *ὀστρακόδερμα* (Schaltiere, Muscheln, Schnecken etc.).
  4. *ἔντομα* (Insekten, inkl. Spinnen, Würmer).

Was zunächst die Einteilung in Bluttiere und Blutlose betrifft, so ist die volle Bedeutung der Tatsache aufrecht zu erhalten, dass *Aristoteles* bereits *Demokrit* von Blutlosen reden lässt (No. 2, III. 4. 1.). Hierauf haben ***Brandis*** und *J. V. Carus* (No. 5, p. 77) die Möglichkeit ausgesprochen, dass diese Unterscheidung voraristotelisch sein könne, wenn es auch noch nicht gelungen ist, *Zellers* Einwand hiegegen (No. 22, II. 2. p. 554) zu entkräften. Jedenfalls darf Aristoteles nicht zum Urheber dieser Einteilung gemacht werden. Die grossen

Gruppen (γένη μέγιστα) der Bluttiere sind gegeben und berühren den Zweck unserer Arbeit nicht. In der Reihenfolge der drei Gruppen von Blutlosen schwankt übrigens *Aristoteles*. So zählt er sie IV 1 auf: 1. Weichtiere. 2. Weichschaltiere. 3. Schaltiere, während bei Anlass der Eiablage und der Begattung er sie einander umgekehrt folgen lässt (No. 1, IV. 61—80, 81—86, 87—91 und VIII. 172).

III. Der zoologische Teil der hippokratischen Schrift περὶ διαίτης.

Treten wir nun mit diesen Feststellungen an die Schrift περὶ διαίτης heran. Nach *Fredrich* (No. 7, p. 172) ist das zweite Buch „für das ganze Altertum das bedeutendste des Werkes gewesen. Es verdient auch gegenüber den andern drei Büchern diese Hervorhebung, weil es ausser den von dem Autor von περὶ ἀρχ. ἰητρικῆς und Diokles getadelten, hypothetischen Erklärungen eine Menge praktischer Beobachtungen verwertet. Ich sage: verwertet, denn diese waren wohl alle schon von andern gemacht worden (τὰ μὲν προειρημένα πάντα εἴρηται), sie sind hier nur bequem zusammengestellt und mit jenen Erklärungen nach der Feuer-Wassertheorie versehen worden.“ Als eine solche Zusammenstellung erweist sich denn auch das Verzeichnis der diätetisch wichtigen Tiere, welches wir der in dieser Schrift eingehaltenen Reihenfolge gemäss nunmehr folgen lassen und zwar im Wortlaut der *Fuchs*'schen Übersetzung (No. 8, I p. 322):

Kapitel X (XLVI).

Über die Tiere, welche man isst, hat man folgendermassen zu urteilen. Rindfleisch ist kräftig, verstopfend und schwer verdaulich für den Magen, weil dieses Tier

dickblütig und vollblütig ist; sein Fleisch, die Fleischfasern an sich, das Blut und die Milch sind dem Körper gegenüber schwer. Bei denjenigen Tieren hingegen, deren Milch fein und deren Blut ebenfalls fein ist, hat das Fleisch die gleiche Eigenschaft. Ziegenfleisch ist leichter als dieses und führt mehr ab. Schweinefleisch verleiht dem Körper zwar mehr Kraft als dieses, führt aber ziemlich heftig ab, weil das Schwein feine und wenig blutreiche Adern, dafür aber viel Fleisch hat. Lammfleisch ist leichter als das Fleisch von Schafen und das des Ziegenbocks leichter als das der Ziege, weil es weniger Blut enthält und feuchter ist. Das Fleisch von Natur trockner und kräftiger Tiere führt, solange die Tiere zart sind, ab, wenn sie hingegen herangewachsen sind, tut es das weniger. Mit dem Kalbfleische verhält es sich dem Rindfleische gegenüber genau so. Das Fleisch von Ferkeln ist schwerer als das von Schweinen, denn da das Tier von Natur viel Fleisch hat und blutarm ist, hat es, solange es noch jung ist, einen Überschuss an Feuchtigkeit; wenn demnach die Poren die hinzukommende Nahrung nicht aufzunehmen vermögen, so verbleibt das Fleisch im Magen und verdirbt ihn. Eselsfleisch führt ab, das Fleisch junger Esel noch mehr. Pferdefleisch ist leichter. Hundefleisch macht warm und trocken und verleiht Kraft, führt jedoch nicht ab. Fleisch von jungen Hunden macht feucht und führt ab, befördert aber mehr die Urinausscheidung. Fleisch vom Wildschweine macht trocken, kräftigt und führt ab. Hirschfleisch macht trocken und führt den Stuhl weniger, den Urin mehr ab. Hasenfleisch ist trocken und verstopfend, bewirkt aber eine gewisse Beschleunigung der Urinsekretion. Fuchsfleisch ist feuchter und wirkt anregend auf die Harnabsonderung. Auch

das Fleisch des Landigels führt den Urin ab und macht feucht.

Kapitel XI (XLVII).

Mit den Vögeln steht es folgendermassen. Das Fleisch der Vögel ist fast ohne Ausnahme trockner als das der Vierfüssler, denn diejenigen Tiere, welche keine Blase haben, weder Urin noch Speichel absondern, sind durchaus trocken. Wegen der Wärme des Leibes nämlich wird das Feuchte aus dem Körper als Nahrung für das Warme aufgebraucht, sodass das Tier weder uriniert noch Speichel secerniert. Wer aber solche Feuchtigkeit nicht in sich hat, der muss notwendiger Weise trocken sein. Am trockensten scheint das Fleisch der wilden Holztaube zu sein, an zweiter Stelle das der zahmen Taube, an dritter Stelle das des Rebhuhns, des Hahns, der Turteltaube, am feuchtesten aber das der Gans. Diejenigen Vögel, welche Körner aufsammeln, sind trockner als die übrigen. Das Fleisch der Ente und aller der übrigen Tiere, welche in Sümpfen oder Wässern leben, ist ohne Ausnahme feucht.

Kapitel XII (XLVIII).

Von den Fischen sind folgende am trockensten: der Drachenkopf, der Drachenfisch, der rauhe Sternseher, der Knurrhahn, der Schattenfisch, der Barsch, die Thrissa; leicht sind fast alle in der Nähe von Felsen lebenden Fische, z. B. der grüne Klippfisch, die schwarze Meergrundel, die Elephitis und der Kaulkopf. Diese und die vorgenannten Fische sind leichter als die Wanderfische, denn da sie sich ruhig verhalten, haben sie ein lockeres und leichtes Fleisch. Die Wander-

fische hingegen, welche durch die Wellen verschlagen werden und durch die Anstrengung ermatten, haben ein härteres und dickeres Fleisch. Der Zitterrochen, der Stachelrochen, die Steinbutte und dergleichen sind leichter. Diejenigen Fische aber, welche ihre Nahrung in schmutzigen Wässern suchen, wie z. B. die Meeräsche, der Pfriemfisch und der Aal sind schwerer, weil sie ihre Nahrung aus dem Wasser, dem Schmutze und dem darin Wachsenden nehmen, deren blosse Ausdünstung schon, wenn sie dem Menschen entgegenkommt, ihn schädigt und ihm Beschwerden macht. Die Fluss- und Teichfische sind noch schwerer als diese. Die Seepolypen, die Tintenfische und dergleichen sind weder leicht, wie es scheint, noch abführend, aber sie schwächen die Augen; ihr Saft dagegen führt ab. Was die Schalentiere anbelangt, wie z. B. die Steckmuschel, die Purpurschnecke, die Napfschnecke, die Trompetenschnecke, die Auster, so macht das Fleisch an sich trocken, der Saft dieser Tiere aber führt ab. Miesmuscheln, Kammmuscheln und *τελλῖναι* führen mehr als diese ab, am meisten aber die Meernesseln. Auch die Knorpelfische machen feucht und führen ab. Die Eier der Seeigel und der feuchte Bestandteil der Krabbe führt ab, nicht minder tun das die *ἄρκοι* und die Krebse, und zwar in höherem Grade die Flusskrebse, doch auch die Seekrebse, zugleich befördern sie die Urinsekretion. Eingesalzene Fische machen trocken und schwächen, besonders führen die fetten ab. Am trockensten von den eingesalzenen Fischen sind die Seefische, nächstdem die Flussfische, am feuchtesten aber die Teichfische. Von den Seefischen selbst wieder sind die sogenannten Barsche am trockensten, sowohl frisch, als auch eingesalzen.

Kapitel XIII (XLIX).

Von den Haustieren sind diejenigen, welche in den Wäldern und auf den Feldern ihre Nahrung suchen, trockner als die im Hause aufgezogenen, weil sie bei dieser Anstrengung sowohl durch die Kälte trocken gemacht werden, als auch eine trocknere Luft einatmen. Die wilden Tiere sind trockner als die zahmen, desgleichen die Rohes und Laub fressenden und die Wenigfresser als die Vielfresser, desgleichen diejenigen, welche Trockenfutter fressen, als diejenigen, welche Grünfutter fressen, die Frucht fressenden als die nicht Frucht fressenden, die wenig saufenden als die vielsaufenden, die vollblütigen als die blutlosen und blutarmen, die in der Vollkraft stehenden als die zu alten oder zu jungen, die männlichen als die weiblichen, die nicht verschnittenen als die verschnittenen, die schwarzen als die weissen, die dicht behaarten als die haarlosen. Tiere entgegengesetzter Beschaffenheit sind feuchter. Von den Tieren selbst wieder sind diejenigen Fleischteile, welche am meisten arbeiten, am blutreichsten sind und auf welchen sie liegen, die stärksten, am leichtesten hingegen diejenigen Fleischteile, welche am wenigsten arbeiten, am ärmsten an Blut sind, im Schatten liegen und im tiefsten Innern des Tieres gelegen sind. Von den blutlosen Teilen ist das Gehirn und das Mark am stärksten, am leichtesten sind der Kopf, die Sehnen, die Schamgegend und die Füsse. Bei den Fischen sind die obern Teile am trockensten, die Bauchteile am leichtesten, der Kopf ist wegen des Fettes und des Gehirns feuchter.

Zunächst einige Bemerkungen zur Übersetzung und Erklärung von *Fuchs*. πέρδιξ ist im Anschluss an die Aristoteleskommentatoren (*Sundevall* No. 20, p. 139 und *Aubert* und *Wimmer* No. 1, I p. 104 und 105) besser

mit Steinhuhn zu übersetzen, da das Steinhuhn schon seiner äusseren Erscheinung nach erheblich vom Rebhuhn abweicht und ein wahrer Charaktervogel der Küstenländer des ägäischen Meeres ist, während das Rebhuhn wohl neben ihm, aber viel seltener daselbst auftritt. (No. 18, p. 116 und 189). Unter dem Barsch *πέρκη* braucht, wie mir scheint, nicht notwendig der Flussbarsch verstanden zu sein, da so viele ähnlich gestaltete Küstenfische von ähnlichem Habitus und derselben Familie angehörend, im Mittelmeer vorkommen, schon auch deswegen, weil wie *Fredrich* (No. 7, p. 181 und 182) die *πέρκη* von *Diokles* (Athenaeus VII 305 b; 309 c) zu den *πετραῖοι* gerechnet wurde, was wohl kaum geschehen wäre, wenn der Name *πέρκη* bloss für den Flussbarsch und nicht ebensowohl für die marinen Barsche verwendet worden wäre. Insbesondere scheint mir damit der auch heute noch am Mittelmeer sehr geschätzte Seebarsch (Labrax lupus) in erster Linie gemeint zu sein. Zu Anmerkung 37 von *Fuchs* ist zu notieren, dass Seespinne der Vulgärausdruck für einen Krebs, Carcinus moenas ist; dass er auch für Octopoden gebraucht werde, ist mir nicht bekannt. *Κογχύλια* wäre wohl besser mit Muscheltieren oder Konchylien zu übersetzen, nicht mit Schaltieren. Die Bezeichnung Schaltiere ist von den Aristotelesübersetzern für die *ὀστρακόδερμα* von Aristoteles gebraucht worden, die neben den Muscheltieren, die auch bei ihm *κογχύλια* heissen, noch weitere Tiergruppen umfassen. Vollends darf aber nicht eine Krebsart wie die *ἄρκοι* in Anmerkung 49 als Gattung von Schaltieren bezeichnet werden, da sie in Zusammenhang mit andern Krebsen aufgezählt werden, die insgesamt von den Aristotelesübersetzern als Weichschaltiere (*μαλακόστρακα*, nicht *μαλακόστρακοι*, wie *Fredrich* p. 182 schreibt) bezeichnet sind.

IV. Reihenfolge und Charakter der Tierwelt in der Schrift des Diätetikers.

Stellen wir übersichtlich nochmals die Reihenfolge der in diesen Kapiteln abgehandelten Tiere zusammen.

I. Säugetiere.

1. Rind,		(Paarhufer),	(Haussäugetiere).
2. Ziege,			
3. Hausschwein,			
4. b. Schaf,	a. Lamm,		
[d. Ziege],	c. Ziegenbock,		
[f. Rind],	e. Kalb,		
[h. Hausschwein],	g. Ferkel,		
5. a. Esel,	b. junger Esel,	(Unpaarhufer),	
6. Pferd,			
7. a. Hund,	b. junger Hund,		
8. Wildschwein,		(Wilde Säugetiere).	
9. Hirsch,			
10. Hase,			
11. Fuchs,			
12. Landigel,			

II. Vögel.

1. Wilde Holztaube.
2. Zahme Taube.
3. Steinhuhn.
4. Huhn.
5. Turteltaube.
6. Gans, } (Wasservögel).
7. Ente, }

III. Fische.

1. Drachenkopf,
2. Drachenfisch,
3. Sternseher,
4. Knurrhahn,
5. Schattenfisch,
6. Barsch,
7. Thrissa,
8. Grüner Klippfisch,
9. Schwarze Meergrundel,
10. Elephitis,
11. Kaulkopf,

(Küstenbewohnende Acanthopterygier).

Wanderfische.

12. Zitterrochen,
13. Stachelrochen,
14. Steinbutte,

(Selachier).

15. Meeräsche,
16. Pfriemfisch,
17. Aal,

(Schlammbewohner).

Fluss- und Teichfische.

IV. (Weichtiere.)

1. Polyp.
2. Sepia.

V. (Schaltiere.)

1. Steckmuschel,
2. Purpurschnecke,
3. Napfschnecke,
4. Trompetenschnecke,
5. Auster,
6. Miesmuschel,
7. Kammmuschel,
8. τελλῖναι,
9. Meernesseln,

Konchylien, Muscheltiere.

10. Seeigel.

Knorpelfische.

VI. (Weichschaltiere.)

1. Krabbe (brachyure Krebse).
2. ἄρκοι, } (makrure Krebse).
3. Seekrebs, } Krebs,
4. Flusskrebs,

Bei dieser Aufzählung sind in runde Klammern gesetzt diejenigen Allgemeinbezeichnungen für Gruppen, die nicht in der Schrift selbst gebraucht werden, die sich aber doch aus der Reihenfolge ergeben. In eckigen Klammern stehen die rekapitulierend erwähnten, schon genannten Tierbezeichnungen. Die römischen Ziffern entsprechen *γένη μέγιστα* von Aristoteles; die arabischen denjenigen Vulgärbezeichnungen der Tiere, die wir etwa als Arten oder Gattungen bezeichnen würden; das kleine Alphabet giebt die diätetisch unterscheidbaren Varietäten, die Fleischarten verschiedener Altersstufen wieder. Cursiv gedruckt sind die Gruppenbezeichnungen, welche beim Autor der Schrift selbst vorkommen. Cursiv und eingeklammert diejenigen, die bei ihm nicht in einem Wort zusammengefasst sind.

Gehen wir nun zur Analyse des Tierverzeichnisses selbst über, so sind zunächst die mutmasslichen Gesichtspunkte, die zur Aufstellung einer solchen Reihenfolge führen mussten, in Erwägung zu ziehn.

In erster Linie kam es dem Diätetiker auf Registrierung der zweckmässigen oder schädlichen tierischen Nahrungsmittel an. Das musste gewisse Zusammenfassungen zur Folge haben. In zweiter Linie war für seine Aufzählung der lokale Charakter der zu besprechenden Objekte massgebend und drittens etwa die Berücksichtigung einer seiner Zeit schon bekannten Reihenfolge anstatt einer regellosen Aufzählung, wie wir sie vor ihm, etwa bei *Herodot*, anzutreffen gewohnt

sind. Endlich kommen Rücksichten auf die stilistische Behandlung des Stoffes in Betracht, die auch bei der sorgfältigsten Verarbeitung eines theoretischen Gegenstandes zu praktischen Zwecken unvermeidlich sind.

Rein praktisch diätetischer Art scheint mir der Exkurs, der bei Anlass des Verhältnisses von Schaf und Lamm gemacht wird und der Veranlassung giebt, auch die Jugendformen, resp. das Geschlecht verschiedener Säugetiere in ihrer diätetischen Bedeutung zu erörtern, nachdem die erwachsenen Tiere bereits erledigt waren. Der Gegensatz im diätetischen Charakter der Jugendstadien, sowie die Tradition der Nahrungsmittelwahl erklärt dies hinreichend.

Ebenso entscheidet über die Reihenfolge der Tauben der Grad von Trockenheit ihres Fleisches, sonst würde wohl die zahme Taube vorangestellt worden sein, wie die zahmen Säugetiere den wilden vorangehn.

Die Auster ist gemäss dem günstigeren Grad ihrer diätetischen Wirkung den übrigen Konchylien zuletzt angeschlossen und von den weniger günstigen nahe verwandten Muscheltieren dadurch etwas abgesondert. Dafür, dass die Byssos liefernde Steckmuschel von ihren Verwandten entfernt und vor die Purpurschnecke gestellt ist, mag vielleicht als Grund folgender gelten. Beide haben überhaupt weniger diätetische Bedeutung, waren aber technisch umso wichtiger und wurden daher vielleicht umso eher in Verbindung genannt.

Der Gesichtspunkt der Diätetik tritt auch da hervor, wo zu Ende des zwölften Kapitels die eingesalzenen Fische eine besondere kleine Gruppe bilden, die wir aus unserm Verzeichnis ganz weggelassen haben.

Endlich treten auch diätetische Gründe der Anordnung und Bezeichnung in Verbindung mit andern auf, vor allem mit oekologischen (auf den Wohnort der

Tiere bezüglichen). Hierher gehören die zusammenfassenden Bezeichnungen: Wanderfische, Flussfische und Teichfische, auch die Gruppe der Schlammbewohner, wenn auch ihr oekologisches Merkmal in einem ganzen Satz und nicht in einer zusammenfassenden Bezeichnung ausgedrückt ist.

Eigenartig ist die Stellung der Knorpelfische in der Reihenfolge; davon noch mehr. Ein rein diätetisches System konnte schon deswegen nicht zur Durchführung gelangen, da irgend ein oberflächlich liegender Einteilungsgrund diätetischer Art fehlte. Wir wären ja auch heute mit aller physiologischen Chemie noch in der bittersten Verlegenheit, wenn wir fünfzig Nahrungstiere in ein wissenschaftlich gerechtfertigtes System der Diätetik bringen müssten.

Lassen wir also einmal diesen Anwurf der diätetischen Interessen an die Reihenfolge bei Seite und ebenso die durch sie hervorgerufenen Gruppierungen, so erhebt sich als zweite Frage, welches der allgemeine Charakter der Glieder dieser Fauna sei. Dies ist ein sehr bedeutungsvolles Moment für die Beurteilung der gesamten Reihenfolge.

Als negativer Zug tritt in dieser Aufzählung hervor, dass alle diätetisch bedeutungslosen Tiere von vornherein weggelassen sind. *Fredrich* vermutet (No 7, p. 183), unser Autor habe im Innern gelebt oder man habe die „grossen Meerfische (Delphine [sic! nobis], Thunfisch)“ nicht gegessen. Davon ist wohl die letztere Ansicht die allein richtige. Demgemäss fehlen denn auch alle ungeniessbaren Tiere, wie Wale, kleine Nager, Fledermäuse, Eidechsen, Schlangen, Insekten, Würmer u. s. w., obschon sie bekannt waren, wie aufs deutlichste aus *Herodot* und dem Corpus hippocraticum hervorgeht. Im übrigen setzt sich der gesamte Tierbestand zu-

sammen: 1. aus Haustieren, 2. aus wilden Landsäugetieren und Vögeln, 3. aus der spezifischen Küstenfauna des Mittelmeers und zwar nur deren regelmässigen Vorkommnissen (daher auch der Thunfisch und seine Verwandten fehlen). Ja es springt uns geradezu das lebensvolle Bild des Fischmarktes eines abgelegenen Küstenstädtchens im Mittelmeer vor Augen, wie es auch heute noch hundertfach vorkommt. Diese kleinen Fischmärkte sind konservativer, als die der grossen Hafenstädte und was „man“ isst, ist dort durch zwei Jahrtausende wohl ähnlich geblieben. Von diesem Gesichtspunkte aus möchte ich auch zwei Stellen unseres Autors deuten. Einmal scheinen mir unter der Bezeichnung *τελλίναι* jene kleinen Küstenmuscheln überhaupt zusammengefasst zu sein, wie Arca, Venus u. a., die auch heute auf den kleinen Fischmärkten mehr als auf den grossen zu Haufen getürmt feil geboten werden und die immer noch zu vortrefflich schmeckenden Suppen verarbeitet, aber wegen ihrer abführenden Wirkung mit Vorsicht genossen werden. Auffallend ist ferner der Unterschied, der auch heute noch mit Rücksicht auf die verschiedene Geniessbarkeit der Selachier gemacht wird. Die grossen werden, so viel ich gesehen habe, nur in den grossen Städten gegessen. Als Delikatesse gelten in Neapel Hexanchus und Lamna, auch ist Alopias geschätzt. Die kleinen dagegen, namentlich Scyllium und Mustelus, die man auch auf den kleinen Fischmärkten antrifft, sind nicht beliebt und werden von den nicht Hungers Sterbenden gemieden. Daher spielen die Selachier auch im Marktbestande kleiner Städte nur insofern eine Rolle, als ihre mit Placoidschuppen besetzte Haut zu Chagrin verarbeitet wird. Es fiel mir auf, dass Körbe voll kleiner, abgehäuteter Scyllien und Mustelen feilgeboten werden, da diese Selachier nicht beliebt sind und die

wertvollen grossen nur ausnahmsweise gefangen werden, jedenfalls aber, auch wenn sie von Alters her sollten gegessen worden sein, nicht dem regelmässigen Markt angehören. So scheint es mir auch zu deuten, wenn der Diätetiker die wertvolleren Rochen und den vermeintlich zu ihnen gehörenden Steinbutt an ihrem Ort aufzählt, die Knorpelfische, unter denen er wohl jene kleinen spindelförmigen Haje versteht aber nur als einen diätetisch unbedeutenden Artikel im Anschluss an die Meernesseln kollektiv und flüchtig anführt.

Es wäre ein Irrtum, wollte man sich vorstellen, dass die Aufzählung des Diätetikers wesentlich neue, seinen Zeitgenossen unbekannte Elemente enthalten hätte. Aus *Athenaeus* geht hervor, dass bereits zu einer Zeit, die zweifellos um ein Beträchtliches vor Abfassung der Schrift de diaeta fällt, der „Vater der griechischen Komödie," *Epicharmos* (ca. 470 v. Chr.) eine Fülle der hier aufgeführten Tiernamen (für unsern Zweck kommen nur die Wassertiere in Betracht) gekannt hat. Das nachfolgende Verzeichnis beweist dies zur Genüge:

III	1.	Athenaeus	VII 320 e.
	2.	„	VII 305 c.
	3.	„	VII 382 d.
	5.	„	VII 295 b.
	6.	„	VII 323 c. 319 b.
	8.	„	VII 305 c.
	11.	„	VII 309 d.
	16.	„	VII 319 b.
	17.	„	VII 297 c.
IV	1.	„	VII 318 e.
	2.	„	
	(3.)	„	

V	2.	Athenaeus	III	85	d.
	3.	„	III	85	c.
	4.	„	III	85	d.
	5.	„	III	85	d.
	6.	„	III	85	d.
	8.	„	III	85	e.
VI	1.	„	III	105	b.

Ausserdem wird der Ausfall an solchen Formen unseres Tierbestandes, die bei *Athenaeus* nicht *Epicharmos* zugeschrieben werden, reichlich aufgewogen durch eine Zahl von Tiernamen epicharmischer Fragmente, die wiederum im Verzeichnis des Diätetikers fehlen. Nicht gering ist ausserdem die Anzahl von Tiernamen, die *Archippos* und *Aristophanes* von *Athenaeus* entnommen sind; sie beweist, dass zur Zeit des Diätetikers auch in Athen ein dem seinigen ähnlicher Bestand an Wassertieren mit Namen genannt, ja bereits in den Wortschatz der Komödiendichtung übergegangen war. Wie sollte dies möglich gewesen sein, wenn es sich nicht um anschauliche Typen des marinen Lebens gehandelt hätte, die jedermann geläufig waren?

Wenn wir nun diese diätetischen und lokalfaunistischen Instanzen in Rechnung setzen, so bleibt uns in der Aufzählung des Diätetikers eine Reihenfolge übrig, die nicht eine zufällige sein kann und die zu weiterer Erklärung deswegen gerade herausfordert, weil der Autor sie so selbstverständlich giebt. Es ist eine absteigende Stufenleiter von Lebewesen, die weitgehende Ähnlichkeit zeigt mit der aristotelischen.

Wenn wir auf Grund der populären Kenntnisse heutiger Zoologie diese Tiere ordnen müssten, wir würden kaum wesentlich anders verfahren. Aber nicht nur die Reihenfolge der 52 Tierarten ist eigentümlich,

sondern auch die Zusammenfassung zu grösseren Gruppen. Da nun der Verfasser der Schrift der koischen Schule angehört und er selbst einleitungsweise bekennt, andere Autoren zu verwerten, so schliesse ich, dass ihm ein Vorbild für seine Reihenfolge müsse vorgelegen haben; ja dass es seinen Lesern ein ziemlich bekanntes gewesen sein müsse. Dieses Vorbild, ob es nun in einer selbständigen Schrift niedergelegt war und ob diese von einem Arzt oder Sophisten mag verfasst gewesen sein, möchte ich als das koische Tiersystem bezeichnen und ihm einmal die nun übrig bleibenden Züge der Abschrift des Diätetikers zuschreiben, anderseits seine Beziehungen zur späteren griechischen Systematik der Zoologie beleuchten. Ich unterscheide also zwischen der Reihenfolge des Diätetikers und dem koischen Tiersystem selbst, dessen Teile im nachfolgenden Abschnitt zu sichten sind.

V. Das koische Tiersystem.

Die Säugetiere sind so geordnet, dass der Anfang mit den Haussäugetieren gemacht wird (I 1—7); unter diesen sind die Zweihufer an die Spitze gestellt (1—4), es folgen die beiden Einhufer (5—6). Diese Einteilung scheint sich in Zusammenhang mit der Tierzucht in Westasien ausgebildet zu haben, wofür ja auch die Einteilung der Säugetiere im Pentateuch spricht (III. Buch Mose 11 und V. Buch Mose 14), wie weit aber diese Unterschiede mit andern anatomischen Eigentümlichkeiten ihrer Träger verbunden wurden, das beweist die von *Gomperz* (No. 11, I p. 253) so hoch geschätzte Stelle in der Schrift über die Gelenke, wo von Zweihufern und Einhufern die Rede ist (No. 8, Bd. III, p. 91). Unter den Haustieren kommt der Hund (7) in letzter

Linie. Ihm folgen die wilden Säugetiere (I 8—12) und zwar etwa der Masse ihres Fleisches nach, also diätetisch geordnet.

Die Vögel imponierten von jeher als eine geschlossene Gruppe. Als anatomisch-physiologische Merkmale werden für sie die Abwesenheit der Blase, sowie das Fehlen von Speichel und Urin angegeben. Leider gelangen auch nur die diätetisch wertvollen zur Behandlung. Nr. II 1—3 sind nur diätetisch angeordnet. Aus der Reihenfolge liesse sich allenfalls schliessen, dass eine Stufenleiter: Flugvögel, Erdvögel, Wasservögel möchte bestanden haben, wovon nur die letztgenannten kollektiv bezeichnet sind.

Höchst beachtenswert ist die Gruppe der Fische. Da wir unter πέρκη, wie p. 385 gezeigt, nicht den Flussbarsch zu verstehen brauchen, so lassen sich III 1—11 als Acanthopterygier der Küste bezeichnen, freilich unter dem Vorbehalt, dass für Thrissa und Elephitis keine Erklärung vorliegt. Die hartstrahlige erste Rückenflosse, sowie überhaupt das stark verknöcherte Skelett galt wohl als Beweis für ihre „Trockenheit.“ Der ganze Bestand erinnert an denjenigen, wie er buntschillernd in demselben Korbe vereinigt auch heute noch vom Mittelmeerfischer pflegt feilgehalten zu werden.

III 12—14 werden zusammengehalten durch einseitige Oberflächenfärbung und abgeplattete Körperform. Dass diese beim Steinbutt anatomisch völlig anders zu deuten ist als bei den Rochen, hat auch später noch lange die Systematiker nicht beunruhigt.

Die übrigen Fische werden durch die oben erwähnten oekologischen und diätetischen Merkmale vereinigt, wobei keine scharf bestimmbare Reihenfolge dieser Gruppen beobachtet wird.

Auf die Fische folgen die Seepolypen, Tintenfische und dergleichen. Es sind hiemit die beiden typischen Gruppen der Cephalopoden bezeichnet, von denen die eine acht gleich lange Arme besitzt (Octapoda), die andere ausser diesen noch zwei besonders gestreckte Haftarme (Decapoda). In zweiter Linie unter den Wirbellosen ist die Gruppe der Konchylien genannt, wobei mit Ausnahme der Steckmuschel, für deren Stellung wir oben (p. 389) eine Vermutung ausgesprochen haben, die Schnecken den Muscheln vorangehn. Die Meernesseln, womit wohl die Seeanemonen gemeint sind, da sie doch an Massenhaftigkeit ihres Auftretens an den mediterranen Küsten alle andern Knidarier weit übertreffen, sind wohl an dieser Stelle nicht mehr der Allgemeinbezeichnung Konchylien untergeordnet. Diese Stelle wird wohl richtiger mit den älteren Herausgebern so gelesen:

αἱ δὲ κνίδαι μάλιστα, καὶ τὰ σελάχεα, ὑγραίνει καὶ διαχωρέει

und nicht wie *Ermerins* will:

αἵ τε κνίδαι μάλιστα· καὶ τὰ etc.

Dabei würden die Selachier, die aus den oben erwähnten Gründen hier nur ganz flüchtig angeschlossen werden, gleichsam in Parenthese eingeschoben sein, was auch besser mit ihrer oben erwähnten Bedeutungslosigkeit als Marktware passen würde, als wenn sie von den Fischen getrennt hier nochmals als Subjekt eines besondern Satzes auftreten würden, wie die neueren Herausgeber annehmen wollen.

Den Schluss der Aufzählung bilden die Seeigel, deren Eierstöcke auch heute noch eine Delikatesse sind, mit der den Neuling vertraut zu machen, Fischern und Schiffern der Mittelmeerküsten eine besondere Freude gewährt. Sodann die Krustentiere, die als Krabbe, *ἄρκος* und zweierlei Krebse, also langschwänzige Kruster,

und zwar Seekrebse und Flusskrebse unterschieden werden.

Damit ist die Reihenfolge des koischen Tiersystems zu Ende und es fragt sich nun, ob sie eine rein zufällige oder eine planmässige sei. Für letztere Auffassung ist erforderlich, dass ihr ein bestimmter Einteilungsgrund innewohnt und ob sich die Absicht einer wissenschaftlichen Systematik darin nachweisen lässt. Wir werden zuerst suchen, ob aus den aufgezählten Tieren sich grössere Gruppen bilden lassen, ob nun diese Gruppen Kollektivbezeichnungen tragen oder nicht. Zu kleineren Gruppen sind vereinigt: I 1—4, I 5—6, III 12—14, III 15—17, IV 1—2, V 1—8, VI 2—4. Zu grösseren Gruppen sind vereinigt: I 1—7 (Haussäugetiere), I 8—12 (wilde Säugetiere), wie denn überhaupt die Säuger in ihrer Gesamtheit. II Vögel, welche unter gemeinsamer Bezeichnung auftreten und auch diätetisch in Zusammenhang mit ihren anatomisch-physiologischen Eigentümlichkeiten behandelt werden. III 1—11 (küstenbewohnende Acanthopterygier), daneben treten die Kollektivbezeichnungen Wanderfische, Fluss- und Teichfische auf, während die ebenfalls kollektiv erfassten III 12—14 (geniessbare Selachier) und III 15—17 (Schlammbewohner) nicht ebenso bezeichnet sind. Unter den Wirbellosen sind allein die Muscheltiere (*κογχύλια*) auch mit einer Gruppenbezeichnung aufgeführt.

Die Reihenfolge für I, II, III ist eine alt hergebrachte. Dagegen ist nicht ebenso selbstverständlich, dass auch die grossen Gruppen der Wirbellosen so zusammengefasst und aneinandergereiht sind, wie es hier geschehen ist. IV entspricht den *μαλάκια* von Aristoteles (Weichtiere, Aubert und Wimmer). V den *ὀστρακόδερμα* von Aristoteles (Schaltiere, Aubert und Wimmer)

VI den μαλακόστρακα don Aristoteles (Weichschaltiere, Aubert und Wimmer). Die Reihenfolge dieser Gruppen aber ist original, wie denn auch ihre Ablösung von den übrigen Wassertieren, die gemeinhin als „Fische“ bezeichnet wurden.

VI. Der Zusammenhang zwischen dem koischen Tiersystem und dem aristotelischen im Einzelnen.

Vergleichen wir nun dieses koische Tiersystem mit andern, so springt sofort in die Augen, dass es in vielen Punkten dem aristotelischen gleich kommt, jedenfalls näher als irgend einem andern, ja dass, wenn wir von jenen durch den Zweck und die Umstände des Autors gegebenen Verschiebungen absehn, es beinahe dasselbe ist. Allerdings bei einer zehnfach geringeren Anzahl der anzuordnenden Tiere. Wie haben wir uns nun die geschichtlichen Beziehungen zwischen dem koischen und dem aristotelischen System vorzustellen?

Hier schienen mir folgende Möglichkeiten offen zu stehn:

Entweder das zweite Buch der Schrift περὶ διαίτης wäre nacharistotelisch und ihr Autor würde im grossen ganzen dem aristotelischen System gefolgt sein, oder die drei Kapitel über tierische Nahrungsmittel wären eine nacharistotelische Interpolation in das zweite Buch der Schrift περὶ διαίτης; dieses selbst hätte dabei doch voraristotelisch sein können. Drittens Aristoteles hätte bei der Abfassung der zoologischen Schriften auf der Reihenfolge von περὶ διαίτης gefusst. Viertens, und dies war das wahrscheinlichste: der Diätetiker und Aristoteles benützten die nämliche Quelle, die später verloren gegangen ist und die das koische Tiersystem enthalten hatte.

Ich wandte mich daher an Herrn Prof. Dr. *Robert Fuchs*, den Verfasser des Abschnittes über die Medizin der Griechen im Handbuch von *Puschmann* (No. 9), den Übersetzer des Corpus hippocraticum (No. 8). Er wies mich in freundlichster Weise auf die Untersuchungen von *Fredrich* (No. 7) und *Poschenrieder* (No. 17) und schrieb mir im Anschluss an meine Darlegungen: „Es ist also kein Zweifel: wir haben die älteste zoologische Systematik vor uns, die wahrscheinlich auch von dem koischen Verfasser von de diaeta entlehnt worden ist und später auf Aristoteles in gleicher Weise überging. Dass die betreffenden zoologischen Kapitel später eingeschoben sein sollten, ist deshalb unmöglich, weil sie sprachlich mit den botanischen vordiokleischen Kapiteln *auf das engste* zusammenhängen.“

Daraufhin sah ich mich nicht mehr genötigt, eine nacharistotelische Einschiebung anzunehmen, da ein so gewiegter Philologe und Hippokrateskenner wie Herr Prof. *Fuchs* sich mit mir über den mutmasslichen Zusammenhang in Übereinstimmung befand. Ich richtete daher meine Bemühungen darauf, die Übereinstimmung zwischen dem Diätetiker und Aristoteles einer nähern Prüfung zu unterwerfen, um einesteils die aus der Reihenfolge der Tiere sich ergebende Schlussfolgerung zu erhärten, anderenteils Anhaltspunkte für die Entwicklung des aristotelischen Systems zu gewinnen. Die Gruppe der flachen und als Nahrungsmittel geschätzten Fische, die wir beim Diätetiker antreffen, lautet nach der Ausgabe von Cornarius (Basel 1538, No. 12, p. 93) *νάρκαι δὲ καὶ ῥίναι, καὶ ψῆσσαι, καὶ τὰ τοιαῦτα ὡς ἐλαφρότερα.* Diese Fischgruppe (III 12—14 unseres Verzeichnisses) kehrt bei Aristoteles wieder (Hist. anim. IX 134, 135); allerdings in etwas zerdehnter, durch Einschiebungen unterbrochener Folge (No. 1, II. p. 268 und 269): *ἡ δὲ νάρκη*

φανερά ἐστι καὶ τοὺς ἀνθρώπους ποιοῦσα ναρκᾶν· καθαμμίζουσι δ'ἑαυτὰ καὶ ὄνος καὶ βάτος καὶ ψῆττα καὶ ῥίνη.... Hierbei ist *ὄνος* ein bisher ungedeuteter Fisch, *βάτος* augenscheinlich einer der zahlreichen Rochen, sei es nun Rhinobatus oder Laeviraja. Jedenfalls handelt es sich in beiden Fällen um dieselbe Gruppe von wohlschmeckenden Flachfischen, der beide Autoren die anatomisch abweichende, nicht zu den Rochen gehörige *ψῆττα* zuzählen.

Noch merkwürdiger aber scheint mir eine andere Übereinstimmung zu sein, die gerade, weil sachlich unbedeutend, umso schlagender ist. Der Autor *περὶ διαίτης* schreibt nach *Cornarius* No. 12, p. 94:

τῶν δὲ ἐχίνων τὰ ᾠὰ καὶ τὸ ὑγρὸν καράβων μύες καὶ ἄρκοι καὶ καρκινοι (wohl aus Versehen accentlos!) *μᾶλλον μὲν οἱ ποτάμιοι, ἀτὰρ καὶ οἱ θαλάσσιοι διαχωρέει καὶ οὐρέεται.*

Daraus macht *Ermerins* No. 13:

Τῶν δὲ ἐχίνων τὰ ᾠὰ καὶ τὸ ὑγρὸν καράβων διαχωρέει, 'καὶ οἱ καρκῖνοι, μᾶλλον μὲν οἱ ποτάμιοι, ἀτὰρ καὶ οἱ θαλάσσιοι διαχωρέουσι, καὶ διουρέονται und fügt die Anmerkung bei: Ante vulgatum *καὶ καρκῖνοι* vulgo *μύες καὶ ἄρκοι* (*ἄρκτοι* E. H. K. Zuing). Littreus *μύες* omisit, equidem *καὶ ἄρκοι* e vicino *καρκῖνοι* ortum esse puto, quare *μύες καὶ ἄρκοι* cum *θ* omitto. —

Fuchs hat sich augenscheinlich von diesem summarischen Verfahren nicht bestechen lassen und übersetzt wenigstens nach *Littré*, der das rätselhafte *ἄρκοι* stehn liess:

„nicht minder tun das die *ἄρκοι* und die Krebse und zwar in höherem Grade die Flusskrebse, doch auch die Seekrebse...." Zu *ἄρκοι* macht er die Anmerkung: Eine bisher nicht bestimmte Gattung von Schaltieren.

Fredrich endlich schreibt nur anmerkungsweise:

„Vor καρκῖνοι steht in den schlechten Handschriften καὶ ἄρκοι; ϑ lässt diese „unbestimmte Gattung von Schaltieren (Fuchs)," die durch Dittographie entstanden ist, fort."

Er acceptiert also die *Ermerins*'sche Streichung und zitiert die *Fuchs*'sche Anmerkung ungenau, die ausserdem, wie oben (p. 385) bemerkt, auch sachlich nicht richtig ist. Denn die ἄρκοι wären ja im aristotelischen Sinne, wie die übrigen Krebse, Malakostraken (Weichschaltiere) und nicht Ostrakodermen (Schaltiere).

Nun hat mich die Unklarheit des Ausdrucks ἄρκοι veranlasst, bei Aristoteles zu suchen, ob etwa dieselbe Bezeichnung sich bei ihm finde.

Im Index verborum von *Aubert* und *Wimmer* (No. 1) figuriert p. 389 das Wort ἄκτοι, μαλακόστρακα 5, 86. Im Tierverzeichnis derselben Autoren (!) steht dagegen p. 152: „1. ἄρκτος. Da von ihm gesagt wird, er laiche zu etwa den gleichen Zeiten wie die κάραβοι, er auch bei andern Schriftstellern nicht vorkommt, ist er völlig unbestimmbar, wie auch schon Cuvier l. c. p. 16 findet."

Bei der Verzweiflung, die das fatale Wort den Herausgebern und Auslegern von *Hippokrates* sowohl wie denen von *Aristoteles* bereitet hatte, war ich nicht wenig überrascht, an der angegebenen Stelle der Tiergeschichte folgenden Wortlaut anzutreffen:

„τοῖς δὲ χρόνοις παραπλησίως καὶ αἱ καλούμεναι ἄρκτοι τίκτουσι τοῖς καράβοις."

Was *Aubert* und *Wimmer* übersetzten (No. 1, p. 501): „In den Zeiten des Eierlegens stimmen die sogenannten „Arktoi" (Bären) mit den Langusten überein."

In den grossen Wörterbüchern von *Stephan* (No. 19) und *Passow* (No. 15) findet sich die hippokratische Stelle erwähnt. Die Lexikographen scheinen keinen Anstoss daran zu nehmen, dass die Schreibweise ἄρκος

durch Auslassung des τ entstanden sei. Wie leicht ein solches Versehen möglich ist, das lehrt die Schreibweise ἄρτος, die bei *Mnesimachos* auch für ἄρκτος vorkommen soll, sowie das Versehen in *Aubert* und *Wimmers* Index verborum.

Jedenfalls aber deutet die Zusammenstellung der beiden Krebsarten beim Diätetiker und bei Aristoteles einmal darauf hin, dass die Konjektur von *Ermerins* καὶ ἄρκοι sei durch Dittographie zu erklären, zum mindesten durchaus überflüssig war. Fernerhin aber darauf, dass gerade die gemeinsame Aufzählung von κάραβοι, ἄρκοι und καρκῖνοι sowohl beim Diätetiker als bei Aristoteles, bei diesem sogar mit einem diätetischen Fingerzeig versehen, einen sehr bedeutsamen Hinweis auf gemeinsame Quellen in sich schliesst.

Ich würde diesen zwei Argumenten nicht solche Bedeutung beilegen, wenn sie allein stünden. Aber *Poschenrieder* (No. 17) hat eine kritische Übersicht über die weitgehenden Zusammenhänge des aristotelischen Textes über Anatomie und Physiologie des Menschen mit den hippokratischen Schriften gegeben und bewiesen, dass Entlehnungen aus *Hippokrates* bei der Abfassung der aristotelischen Schriften zweifellos müssen stattgefunden haben. Die beiden von mir angezogenen Stellen dürften daher nur im Sinne einer Bestätigung der *Poschenrieder*'schen Ansicht auf anderm Gebiete erscheinen. Nebenbei gesagt, möchte ich auch die Stelle über die Eingeweidewürmer bei *Aristoteles* (H. a. V. 94) als von der betreffenden in der hippokratischen Schrift περὶ νούσων δ' Kap. XXIII entlehnt und verkürzt annehmen.

In diesem Zusammenhange ist nochmals auf die grossen Gruppen der Wirbellosen zurückzukommen. Im koischen Tiersystem treten sie begrenzt, aber mit

Ausnahme der Muscheltiere nicht kollektiv bezeichnet auf. Die Bezeichnung κογχύλια ist noch nicht durch die wissenschaftliche ὀστρακόδερμα ersetzt. Es fehlen die Bezeichnungen μαλάκια und μαλακόστρακα. Daraus lässt sich schliessen, dass das System prodiokleisch sei, denn sonst würden wohl bei der sonstigen Neigung des Diätetikers, Gruppenbezeichnungen der verschiedenen Fischgruppen (Wanderfische, Flussfische, Teichfische, Knorpelfische) zu brauchen, die Bezeichnungen für die Gruppen der Wirbellosen zur Verwendung gelangt sein, wenn sie bereits wissenschaftlich fixiert gewesen wären. μαλάκια nannte schon *Diokles* die Cephalopoden; die Allgemeinbezeichnung κογχύλια konnte auch ihm noch genügen, wie sie im koischen System angewandt wurde; an der einen von *Athenaeus* (III 91 d) zitierten Stelle kommt sie aber nicht einmal vor, sondern es werden κόγχαι aufgezählt und dahinter spezifiziert, an der andern (III 86 b) wird sie von ihm gebraucht. Für die ὀστρακόδερμα des Aristoteles gebraucht (III 105 b) aber Diokles noch keinen besondern Namen, sondern führt sie einzeln auf.

Die Bildung grösserer Gruppen der Wirbellosen scheint sich also folgendermassen vollzogen zu haben: Im koischen System waren da: die Abteilungen der Weichtiere, Schaltiere und Weichschaltiere. Mit Ausnahme der Schaltiere (κογχύλια) trugen sie noch keine Gruppenbezeichnungen. Bei *Diokles* finden wir die Weichtiere zuerst unter der Bezeichnung μαλάκια, die κογχύλια (κόγχαι) hat er mit übernommen. *Aristoteles* stellt diese mit einigen weiteren Gruppen niederer Wirbelloser unter der Bezeichnung ὀστρακόδερμα zusammen und verwendet κογχύλια für die solid beschalten Lamellibranchier und Gasteropoden; ihnen stellt er die μαλακόστρακα gegenüber, für die auch *Diokles* noch

keine Bezeichnung hatte. Noch bei *Aristoteles* schwankt ihre Reihenfolge. Neu hinzu kommen die ἔντομα, die wohl auch im koischen System nicht gefehlt haben, von denen aber aus begreiflichen Gründen der Diätetiker nicht berichtet. So bestehen denn die wesentlichsten Unterschiede zwischen dem koischen und dem aristotelischen System in drei Punkten: 1. in der Umstellung der μαλακόστρακα und ὀστρακόδερμα, 2. in dem Fehlen der Gruppennamen, 3. in der Verschiedenheit des Gebrauchs von κογχύλια für eine weitern Gruppe im koischen, eine engere im aristotelischen System. Das Fehlen der ἔντομα findet in den Absichten des Diätetikers seine volle Erklärung.

Wie verhält es sich nun mit der zeitlichen Aufeinanderfolge der Schrift des Diätetikers, der Fragmente des *Diokles* und den zoologischen Schriften von *Aristoteles?* Hier hat wohl das erste Wort die Philologie und Quellenkritik. In Betreff des *Aristoteles* herrscht kein Zweifel. Etwa 335/34 kam er nach Athen und da entstanden seine Hauptschriften binnen 12 Jahren (Zeller II 2, p. 28). Die Tiergeschichte fällt also auf ca. 330. Als Abfassungszeit des II. Buches von περὶ διαίτης nimmt *Fuchs* (No. 9) an ca. 400. *Diokles* wird von ihm wegen seiner Polemik gegen „die offenbar noch nicht lange vorliegende pseudohippokratische Schrift de diaeta“ in den ersten Teil des IV. Jahrhunderts verlegt (also zwischen 400 und 350). Mag nun immerhin die Abfassung von περὶ διαίτης um das Jahr 400 stattgefunden haben, so ist doch anzunehmen, dass das koische Tiersystem, das vielleicht in einer besonderen Schrift niedergelegt war, damals schon eine bekannte Schrift älteren Datums war, also wahrscheinlich vor 400 zu datieren ist. Ja der Reichtum von denselben Tiernamen be *Epicharmos* und dem Diätetiker legen der Annahme

kein Hindernis in den Weg, dass das koische System bereits zu Beginn des fünften Jahrhunderts könnte vorhanden gewesen sein, da der Komiker schwerlich so zahlreiche Namen verwenden durfte, wenn nicht die Begriffe seinen Hörern geläufige waren.

VII. Die knidische Tierfolge.

Bisher habe ich die Frage gar nicht berührt, ob auch in andern hippokratischen Schriften ähnliche Tieraufzeichnungen vorkommen. In so ausführlicher Weise, wie beim Diätetiker, ist dies allerdings nirgends der Fall. Doch kommt ein bedeutend kleineres und weniger geordnetes Tierverzeichnis in diätetischem Zusammenhang in der Schrift *περὶ παθῶν* (No. 8, II. p. 373) vor. Die Reihenfolge ist hier: Hund, Geflügel, Hase (in dieser Verbindung diätetisch zusammengestellt), Rind, Schwein (ebenso), Schaf, (Schwein); gegenübergestellt ist dieser Reihe Wildpret. Dann folgt ein Satz über die Abhängigkeit der diätetischen Bedeutung des Fleisches von der Ernährung des Schlachttieres. Fische: a. im Allgemeinen, b. im Besondern unterschieden in Teichfische, fettere Fische, Flussfische, Küstenfische, Meerfische. Der wissenschaftliche Charakter dieser Aufzählung ist etwa folgendermassen zu präzisieren:

In Bezug auf Spezifizierung ist sie weit ärmer als das koische Tiersystem. Die Reihenfolge, die bei der geringen Zahl von Elementen noch leichter systematisch richtig sich gestalten liess, ist weniger konsequent, wenn das Geflügel zwischen die Säugetiere eingeschoben, der Hase nicht zum Wildpret gezählt ist. In Bezug auf Gliederung ist in ihr ein ursprünglicher Zustand dadurch gewahrt, dass als Fische noch alle Wassertiere bezeichnet sind, ferner dass ein Einteilungsgrund nur

bei den Fischen zu finden ist und zwar der Ort des Vorkommens, also ein oekologischer, kein anatomischer, wie man ihn in der solidarpathologisch und daher anatomisch veranlagten knidischen Schule am ehesten anzutreffen erwartet hätte. Die einzige Spur systematischer Absichten lässt sich darin erblicken, dass die nach Wohnorten unterschiedenen Fischgruppen wenigstens geographische Konsequenz verraten, wie wir sie beim koischen Tiersystem vermissen.

Aus alledem scheint mir hervorzugehn, dass diese Aufzählung älter und weniger durchgebildet ist als die koische und der Betonung systematischer Absichten entbehrt. Ich möchte sie daher nicht mehr als System, sondern einfach als die knidische Tierfolge bezeichnen.

Über ihr Alter mag die Philologie entscheiden. Nach *Fuchs* (No. 9, p. 214) wäre die betreffende Schrift für Laien verfasst und knidischen Ursprungs. Nun soll aber der Verfasser von *περὶ παθῶν* dem ebenfalls knidischen von *περὶ νούσων α'* sehr nahe stehn, dieser selbst wieder seine Schrift nach *περὶ ἀέρων, ὑδάτων, τόπων* gearbeitet haben, die endlich als echt hippokratisch gilt, d. h. Hippokrates II. (dem Grossen, geb. ca. 460) zugeschrieben wird. Da nun aber die letztgenannte Schrift zwischen 420 und 406 angesetzt wird, müsste, wenn diese Bestimmungen annähernd richtig sind, zwischen *περὶ ἀέρων* etc. und *περὶ διαίτης* II entstanden sein: *περὶ παθῶν* und *περὶ νούσων α'*, erst dann das koische Tiersystem, das selbst wiederum *περὶ διαίτης* II voranging. Damit würde die p. 405 gemachte Annahme eines höheren Alters für das koische Tiersystem wiederum in Zweifel gezogen.

VIII. Zur Urgeschichte der systematischen Prinzipien.

Die knidische Tierfolge besitzt aber nicht nur das Interesse, weit genereller und unvollkommener zu sein als das koische Tiersystem, das mit ihr keinerlei nähere Berührungspunkte aufweist; sie erinnert in ihrer Einfachheit und Allgemeinheit viel mehr an Aufzählungen, wie uns eine solche etwa im Dekalog des I. Buches Mose erhalten ist, wo die Tiere ebenfalls nach dem Einteilungsprinzip des Mediums unterschieden werden. Damit ist der Punkt erreicht, auf dem die Zoologie der Urvölker steht: die ganze leblose Natur wird mit Dämonen verphantasiert, das lebende Wesen aber wird als blosses Möbel in der Weltwirtschaft aufgefasst.

Die Vervollkommnung in der Betrachtung und Ordnung der Tierwelt, die sich von der knidischen Tierfolge bis zum koischen Tiersystem vollzieht, besteht wesentlich darin, dass einmal die Reihenfolge des Diätetikers bereits eine weit grössere Zahl von Einzelobjekten innerhalb grösserer Gesamtbegriffe vereinigt und wohl geordnet wiedergibt, dass sie dabei nicht nur nach oekologischen und diätetischen Einteilungsprinzipien verfährt und dass sie eine ganze Reihe von Wirbellosen, zum erstenmale von den Fischen im engeren Sinne abgetrennt, nach anatomischer Verwandtschaft ordnet. Gerade in der Betonung und Abtrennung der Wirbellosen aber ist ein Hauptfortschritt des koischen Tiersystems zu sehen. Im koischen System erst ist die Mannigfaltigkeit der Lebewelt zum Forschungsobjekt und die Einheit der anatomischen Übereinstimmung zum Einteilungsprinzip erhoben.

In diesem Zusammenhange habe ich jetzt erst auf das XIII. Kapitel des Diätetikers einzugehen (vergl. oben

p. 384). Hier erscheint eine Aufzählung des Nahrungswertes der Tiere: *1. Nach dem Grad der Domestikation:* b. halbwilde Haustiere, a. echte Haustiere, c. wilde Tiere. *2. Nach der Ernährungsweise:* a. Rohes und laubfressende, b. Wenigfresser, c. Vielfresser, d. Trockenfutter fressende, e. Grünfutter fressende, f. Frucht fressende. *3. Nach dem Verhalten inbezug auf Flüssigkeiten:* a. Aufnahme von Wasser, *α*. wenig, *β*. viel, b. Gehalt an Blut: *α*. vollblütig, *γ*. blutarm, *β*. blutlos; *4. nach den Lebensaltern:* b. Vollkraft, c. alt, a. jung, 5. *nach Geschlecht:* a. männliche, b. weibliche, c. verschnittene. *5. Äussere Bedeckung:* a. Farbe, b. Behaarung.

Dieser Aufzählung schliesst sich eine solche an, die wir als anatomische bezeichnen könnten und die von den vermeintlichen Leistungen des Fleisches, aktiven und passiven ausgehend, aus der Oberfläche nach der Tiefe des Körpers vordringt. Die Verallgemeinerung der Ansicht, dass Blutgehalt und Topogrophie in correlativem Verhältnis stehen, führt zu der Annahme der Blutlosigkeit für die innersten Organe, Gehirn und Rückenmark. Auch für die Fische wird noch kurz eine topographische Diätetik aufgestellt.

Hiebei treten verschiedene bemerkenswerte Punkte zutage. Dieses Kapitel zeigt uns ein ebenso grosses systematisches Talent an der Arbeit, wie die vorangehenden. Der Diätetiker war es kaum selbst; aber er bindet sich wohl an ein übernommenes biologisches System, dessen Einteilungsprinzip in der ersten Hälfte ein physiologisches ist und gemäss der starken Betonung der geographischen Physiologie bei den Hippokratikern ein vorwiegend geographisch-physiologisches. In der zweiten Hälfte ist es die Topographie innerhalb des tierischen Individuums, die als Einteilungsprinzip figuriert. Das ganze Kapitel vermag nur den Eindruck zu verstärken,

dass auch in anderer Richtung hier die Wurzeln biologischer Systematik zu suchen sind und dass bereits dem Diätetiker und seinen Lesern neben der Einteilung der organischen Naturwissenschaften in Botanik und Zoologie eine Dreiteilung der letztern geläufig war in Zoologie, Physiologie und Anatomie. Demgemäss darf man sich die Entwicklung der biologischen Systeme bereits vor dem Diätetiker nicht etwa unbedeutend vorstellen; welche Fortschritte weiterhin in der koischen Schule noch gemacht worden sind, entzieht sich der Beurteilung und wir erinnern nur nochmals an *Diokles*, von dem unter den bekannten Koern wir uns am ehesten Bemühungen zur Erweiterung und Ordnung des biologischen Tatbestandes vorstellen könnten.

Nur eine voraristotelische Tierfolge ist ausserdem bekannt, die platonische zu Schluss des Timaeus (No. 16, p. 220—222), wo der Mensch zum Weib, zum Vogel, zum vierfüssigen und zum vielfüssigen Tiere degeneriert, wo endlich die Füsse verloren gehen, die Wassertiere entstehen, die Fische zuerst, dann die Schaltiere. Dem wirklichkeitsfremden *Plato* kann, da auch sonst der Timaeus nicht die geringste Vertrautheit mit der Beobachtung organischer Naturerscheinungen aufweist, unmöglich eine selbständige Aufstellung solch einer Succession zugemutet werden. Sie erscheint vielmehr in ihrer ironischen Fassung als eine Parodie auf sophistische Traditionen, deren Ursprung wir wohl in demselben Kreise zu suchen hätten, dem sie auch der Verfasser von *περὶ διαίτης* entnommen hat. Damit aber verrät sie immerhin, dass der tierischen Stufenleiter schon vor *Aristoteles* eine allgemeine Bedeutung beigelegt worden ist.

Und nun *Aristoteles* selbst. Die von ihm zu bewältigende Fülle der Tierformen war im Vergleich zu der des Diätetikers die zehnfache; lag jedoch ein koisches

Tiersystem vor, so dürfen wir den Unterschied zwischen der Artenzahl bei *Aristoteles* und der jenes Tiersystems nicht so hoch einschätzen, da doch schon *Herodot* etwa 70 Landtiere erwähnt und der hypothetische koische Systematiker hievon wohl ebenfalls einen grossen Teil, ausserdem aber die Wassertiere in grösserer Anzahl kannte, aus diätetischen Gründen aber nicht zu erwähnen hatte. Die grossen Gruppen übernahm *Aristoteles* aus dem koischen Tiersystem, wie wir oben (p. 404) wahrscheinlich zu machen suchten; den Gedanken einer Succession der Lebewesen ebenfalls. Das wesentliche Merkmal seiner Systematik ist also wohl nicht in der speziellen Anordnung des Stoffes zu suchen, sondern vielmehr in seiner Entwicklung der logischen Prinzipien für die Systematik. Hiefür ist von fundamentaler Bedeutung, dass er der erste uns bekannte Bekämpfer der physiologischen Einteilungsprinzipien ist, die er durch anatomische mit voller Absicht ersetzt. Sollte man finden, dass seine Verdienste um die zoologische Systematik durch den Nachweis des koischen Tiersystems gemindert würden, so ist darin ein Gegenwert zu erblicken, der ihm kaum kann bestritten werden. Das logische Instrument also, das vor ihm bestenfalls instinktiv gehandhabt wurde, er wendet es bewusst an, wenn er (de partibus anim. I. 3. 643 a 35) sagt, man dürfe die beseelten Wesen nicht nach gemeinsamen Verrichtungen einteilen, auch nicht nach dem Vorkommen in Land- und Wassertiere (de partibus anim. 144 b 32 und *J. B. Meyer* (No. 14, p. 89 ff.) Um die Bedeutung dieses Prinzips für die zoologische Systematik zu ermessen, hat man sich zu vergegenwärtigen, dass sich in ihm ein Prozess zu vollziehen begann, der die ganze Entwicklung der zoologischen Systematik in der Folgezeit bis zur Gegenwart, ja noch auf lange hinaus kennzeichnet. Es ist hiefür

nebensächlich, ob wir der anatomischen Begründung der Systematik eine rein logisch-ideale oder eine genetisch-reale Deutung verliehen. Um nicht über den Vorstellungskreis der heutigen Zoologie weiter hinauszugreifen, will ich nur einige bekannte Beispiele aufführen, die moderne Anwendungen desselben Prinzips sind, wonach *Aristoteles* die Wale für Säugetiere, die Vögel für Verwandte der Reptilien, erklärte. In der zweiten Hälfte des XIX. Jahrhunderts sind aufgelöst worden die Waltiere in Wale und Sirenen, deren letztere den Huftieren angeschlossen wurden (*Owen*), die Waltiere selbst wiederum in zwei getrennte Abteilungen von verschiedener Abkunft: Zahnwale und Bartenwale (*Kükenthal*). Die unter dem Namen Edentaten vereinigten Säuger hat *Flower* in die anatomisch geschiedenen Nomarthra und Xenarthra zerlegt. Ein klassisches, hier nicht näher auszuführendes Beispiel ist die Entstehung und Wiederauflösung der *Cuvier*'schen Pachydermata. Die ehemalige Ordnung der Laufvögel haben *Owen* und *Fürbringer* endgiltig aufgelöst und die verschiedenen fluglosen Vögel als Abkömmlinge von einstigen Fliegern ihren näheren Familienverbänden zugewiesen, denen sie dem Augenschein nach fern standen und neben denen sie eine gemeinsame Ordnung zu bilden schienen. Als Beispiel aus den Wirbellosen sei erwähnt die Abtrennung der Brachiopoden von den Mollusken und ihre Einreihung in die Würmer. Für die Gruppe der Knochenfische sind wir gar nicht imstande, anatomisch begründete Verwandtschaft an Stelle von Anordnungen zu setzen, die physiologischer Gemeinschaft der Merkmale stark verdächtig sind. Ob es jemals gelingen wird, ihre genetische Verwandtschaft festzustellen, muss die Zukunft lehren. Dasselbe gilt aber für die meisten artenreichen Tier- und Pflanzengruppen, die der Scheidung physiologischer

(Anpassungs-)Charaktere und genetischer (Vererbungs-) Charaktere noch bis auf unsere Zeit den grössten Widerstand entgegensetzen.

Einstweilen bleibt also *Aristoteles* der erste, der die Prinzipien der zoologischen Systematik bewusst angewandt hat.

Anderseits erscheint nun aber sein System nicht mehr als die Grundlage und der Anfang der zoologischen Systematik, sondern es ist das Endglied einer langen und langsamen Entwicklung des Denkens über die organische Natur und ihre Mannigfaltigkeit, eines Prozesses, dessen dunkle Spuren immerhin noch zu verfolgen sind. Eine solche Auffassung der aristotelischen Systematik verträgt sich auch mit der von modern philologischer Seite vertretenen Anschauung über die Abhängigkeit des *Aristoteles* von seinen Vorgängern. Ich brauche nur an die Worte von *Diels* (No. 6) zu erinnern: „Wer die Entstehungsweise unserer aristotelischen Lehrbücher kennt“, muss sich sagen, „das hier bereits ein unendliches Material aus einer grossen Bibliothek aufgehäuft sein musste, ehe der Baumeister sein Werk beginnen konnte.“

Von den verschiedenen Tieraufzählungen verdienen eigentlich nur das koische und das aristotelische die Bezeichnung von Systemen, da nur in ihnen die Einzelglieder sich Allgemeinbezeichnungen oder wenigstens Gruppen unterordnen, die selbst wiederum zu einander in einem logischen Verhältnis, nämlich der Stufenleiter der Lebewesen entsprechend angeordnet sind. Um die volle Bedeutung dieser Werke logischer Kunst zu würdigen, ist einmal zu berücksichtigen, wie wenig noch die knidische Tierfolge diesen logischen Anforderungen entspricht, wie spät also erst das koische Tiersystem sich bildete und anderseits, wie rapid nach *Aristoteles* die

Systematik in Verfall geriet, geschweige denn, dass sie bis auf *Linné* hinab einen wesentlichen Ausbau im Sinne von *Aristoteles* selbst erfahren hätte.

Schon in der peripatetischen Schule entstanden jene Machwerke, zu denen nicht nur die falschen Bücher der aristotelischen Tiergeschichte, sondern auch jene von *Athenaeus* aufgeführten *Ζωικά* zu zählen sind, „das Musterbild aller späteren und vielfach nicht eben besonders erquicklichen Schriftstellerei dieser Art“ (*Susemihl* Bd. I, p. 166). Schon bei *Aristophanes von Byzantion* (ca. 257—180 v. Chr.) scheint das Gefüge der aristotelischen Reihenfolge auseinander gegangen zu sein (Ebenda p. 442). *Alexander von Myndos* lief bereits in katalogisches Verfahren mit seinen Tiergeschichten aus und *Athenaeus* (um 228 n. Chr. geb.) war bei der alphabetischen Aufzählung angelangt (VII 277 c.). Wofern nicht weitere Vereinfachung den Nachfolgern zweckmässig schien, genügte die alphabetische Aufzählung, die wir denn auch charakteristisch genug bis auf *Conrad Gesner* (No. 10) verfolgen können. Erst durch ihr Isoliertsein also rücken die beiden Tiersysteme, das koische und das aristotelische ins richtige Licht und bei aller Unvollkommenheit des ersteren wird man ihm diesen Ruhmestitel schwerlich bestreiten können.

Ebenso wohl wie für die zoologische Systematik scheint Aristoteles auch nicht ganz ohne Quellen für die anatomische Systematik gewesen zu sein, da uns doch das XIII. Kapitel der Schrift *περὶ διαίτης* ebenfalls Spuren topographisch-anatomischer Systematik verrät. Sein besonderes Verhältnis zur physiologischen Systematik hier schon darzustellen, würde uns zu weit vom Zweck unserer vorliegenden Studie wegführen.

Basel, Januar 1904.

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6

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Les tendances actuelles de La Morphologie et ses Rapports avec les autres sciences

Conférence faite au Congrès des sciences et arts de l'Exposition de Saint-Louis (U. S. A.), le 21 septembre 1904

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A. Giard

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LES TENDANCES ACTUELLES
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Il y a près de quarante ans, à l'occasion d'une grande manifestation internationale de la pensée humaine telle que celle ou nous sommes conviés en ce moment, dans son *Rapport sur le progrès de la physiologie* (1) publié lors de l'Exposition universelle de Paris en 1867, l'illustre Claude Bernard s'efforçait de démontrer que les sciences doivent se diviser en deux catégories : l'une comprenant l'Astronomie et les sciences naturelles, sciences de *contemplation et d'observation* qui ne peuvent aboutir qu'à la prévision des faits; l'autre dans laquelle il rangeait la Physique, la Chimie et la Physiologie qui seules, disait-il, sont des sciences *explicatives*, *actives et conquérantes de la nature.*

(1) Cl. Bernard. *Rapport sur le progrès de la Physiologie générale en France*, 1867, p. 132 et *Revue des cours scientifiques*, 1869, p. 135 et *passim*.

Cette espèce de contraste établi entre les sciences de la nature dont nous subissons passivement les lois, et celles où intervient l'activité de l'homme est l'expression renouvelée mais considérablement améliorée de l'opinion des philosophes du XVII[e] siècle, Thomas Hobbes en particulier, qui dans son livre le *Leviathan* s'exprimait en ces termes :

« Le registre de la connaissance des faits s'appelle l'histoire et se divise en deux parties : 1° l'histoire naturelle qui a pour objet les faits ou phénomènes de la nature *sur lesquels la volonté de l'homme n'a point prise*, comme, par exemple, l'histoire des métaux, celle des plantes, des animaux ou des contrées, etc. ; 2° l'histoire politique qui expose les actes volontaires des hommes organisés en cité. »

En concédant aux sciences de la nature la capacité de prévoir les faits, Claude Bernard leur faisait, du moins en apparence, une part assez large, car ce qui constitue l'essence même d'une science c'est, comme l'avait déjà reconnu Locke, la prévision de l'avenir et l'on ne peut que répéter avec W. Ostwald, « *the greatest leaders of man have been those who saw most clearly into the future* (1). »

Mais si l'on cherche à mieux pénétrer la pensée du grand physiologiste français, on reconnaît bientôt que le rôle d'observateurs contemplatifs attribué par lui aux savants morphologistes est des plus modestes, en comparaison de celui bien plus élevé qu'il entend réserver aux sciences dites expérimentales ou conquérantes de la nature.

(1) WILHELM OSTWALD. *The relation of Biologie and the neighboring sciences.* (*University of California Publications Physiology*, vol. I, p. 15, oct. 1903).

C'est à celles-ci qu'il appartient à la fois de prévoir à volonté les événements et de les créer au besoin :

Car l'observateur considère les phénomènes dans la condition où la nature les lui offre ; l'expérimentateur les fait apparaître dans les conditions dont il est le maître. Le naturaliste est un descripteur, le physiologiste est un créateur.

Discutable à l'époque où elle fut émise et bientôt discutée en effet par des hommes de grande valeur, la division des sciences proposée par Claude Bernard ne pouvait résister bien longtemps au progrès des idées, si rapide à la fin du XIX^e siècle.

Elle reposait en grande partie sur un malentendu dans la conception du mot expérience auquel il convient de donner, comme nous le verrons plus loin, une signification moins étroite que ne le faisait l'école de Magendie et que ne le font encore aujourd'hui certains physiologistes.

D'ailleurs à ceux qui voudraient voir dans ce débat autre chose qu'une question de mot et un déplacement d'étiquettes, il serait facile de répondre aussitôt par l'histoire des conquêtes dues aux études morphologiques depuis le milieu du siècle dernier.

Dans le domaine si neuf et si peu connu de la cytologie en particulier ne peut-on pas dire que tout ce que nous savons relativement à la question fondamentale de la division cellulaire est le fruit des efforts des morphologistes, que les procédés employés par les histologistes pour élucider le problème de la division cellulaire dépassent de beaucoup l'observation simple et qu'ils ont exigé autant de persévérante ingéniosité, autant d'habileté technique et de connaissances accessoires que n'importe quelle expérience de physiologie pure.

Le triomphe des doctrines de Lamarck et de Darwin, le magnifique mouvement des esprits provoqué dès 1857 par la publication de *l'Origine des espèces*, les controverses de toutes sortes soulevées par la théorie de la descendance modifiée devaient bientôt bouleverser les vues des naturalistes et assigner une signification nouvelle aux recherches de Morphologie comme aux travaux ressortissant aux autres branches de la Biologie.

L'Histoire naturelle pouvait aspirer à son tour au titre de science explicative et conquérante de la nature. Et si cette transformation de l'opinion ne s'est pas opérée plus promptement et d'une façon plus simultanée dans tous les pays de haute culture scientifique, la faute en est pour une bonne part aux naturalistes eux-mêmes, à leur opiniâtreté à conserver les anciens dogmes, à leur défiance à l'égard d'idées géniales dont les auteurs ont été d'abord méconnus dans leurs pays d'origine.

Tant que les biologistes s'obstinaient à soutenir à la suite de Cuvier et de R. Owen que les espèces végétales ou animales sont immuables et que le but suprême de la science est la classification, il est évident que l'histoire des êtres vivants ne pouvait être que la description aussi exacte que possible de leur forme extérieure et de leur structure interne à l'état adulte et à l'état larvaire, la comparaison de ces formes et de ces structures, l'étude des mœurs, c'est-à-dire, les rapports des organismes entre eux et avec les milieux ambiants, la répartition de ces organismes à la surface du globe considérée comme le résultat du caprice ou de l'intelligence d'un créateur omnipotent. En dehors de ses applications pratiques (utilisation par l'homme des produits ani-

maux ou végétaux) l'histoire naturelle pouvait encore donner des jouissances analogues à celles que nous éprouvons à la vue d'un objet d'art, mais d'un art dont la technique nous échappait et dont les procédés demeuraient pour nous une énigme indéchiffrable.

Mais le point de vue changeait du tout au tout, si au lieu de considérer la création à l'état statique comme un ensemble désormais immuable, on l'envisageait au point de vue dynamique, si on étudiait non plus la *natura naturata*, mais la *natura naturans* en cherchant à découvrir les liens de filiation des êtres vivants et à démêler les processus compliqués par lesquels les formes et les organismes se déterminent et se rattachent les uns aux autres; si on cessait d'admirer dévotement les harmonies des animaux et des végétaux soit entre eux, soit avec le milieu qui les environne et de s'en tenir au finalisme enfantin dont Bernardin de Saint-Pierre nous a donné la plus parfaite expression, si, en un mot, on abandonnait la méthode anthropocentrique pour chercher à expliquer comment ces harmonies se sont graduellement établies ou modifiées à mesure que s'établissaient ou se modifiaient les conditions de milieu dans lesquelles elles s'étaient réalisées.

Aussi dès 1877 au Congrès des médecins et naturalistes allemands réuni cette année à Munich, un des premiers et des plus ardents protagonistes de la doctrine darwinienne, Ernest Haeckel, pouvait-il proclamer justement :

« Par la théorie de la descendance, la biologie en général, et spécialement la zoologie et la botanique systématiques, s'élèvent véritablement au rang *d'Histoire naturelle*, titre d'honneur qu'elles portent depuis longtemps mais qu'elles ne méritent que de nos jours. Si ces mêmes sciences sont

encore bien des fois désignées, et cela même officiellement comme sciences naturelles *descriptives* par opposition aux sciences *explicatives*, cela prouve quelle fausse idée l'on s'est faite jusqu'à présent de leur véritable portée. Depuis que le système naturel des organismes est regardé comme l'expression de leur arbre généalogique, la systématique, si sèche dans ses descriptions, fait place à l'histoire plus vivante de la généalogie des classes et des espèces (1). »

Une conséquence plus importante encore découlait de ces conceptions nouvelles. La théorie de la descendance introduisait dans les sciences biologiques une unité de vues, une communauté de but, qui établissaient entre elles les liens les plus étroits de dépendance mutuelle et supprimait toute vaine question de suprématie ou de préséance. Quelles que fussent en effet les méthodes employées, déduction ou induction, observation ou expérience, l'Anatomie, la Physiologie, l'Ethnologie, la Géonémie, la Systématique, la Paléontologie, toutes ces parties d'un ensemble désormais indivisible devaient tendre à la réalisation d'un même programme : retracer d'une façon aussi exacte et aussi complète que possible l'histoire des manifestations de la vie sur notre planète en laissant aux métaphysiciens et aux poètes le soin d'en chercher les origines premières ou d'en célébrer les finalités.

Un coup d'œil rapide nous permettra d'apprécier quels résultats ont déjà été obtenus par ce concours d'efforts convergents et quelles espérances nous pouvons concevoir pour l'avenir, quand, étendant ses frontières, la Biologie bénéficiera des progrès de sciences avec lesquelles elle n'a eu jusqu'à présent

(1) E. Haeckel. *La théorie de l'évolution dans ses rapports avec la philosophie naturelle*. Congrès des naturalistes allemands à Munich. (*Revue Scientifique*, 8 déc. 1877, p. 531)

que des rapports trop lointains. Ainsi tandis que les branches antiques de la Morphologie rajeunies et vivifiées se couvriront d'une frondaison nouvelle, nous verrons apparaître et se développer autour d'elle de nouveaux rameaux gonflés d'une sève généreuse et puissante : la Cytologie, la Promorphologie, la Tectologie, la Morphologie expérimentale (ou création des formes par l'action des facteurs primaires), la Génésiologie, la Biométrique, etc.

Mais le fait même de l'étroite dépendance de ces diverses parties de la science, leurs interférences réciproques, l'intrication des causes qui ont présidé à leur naissance et à leur évolution rendront fréquemment cet exposé difficile et parfois peut-être obscur.

Qu'il me soit permis de m'en excuser d'avance et de réclamer toute l'indulgence de mes auditeurs si je n'ai pu toujours réussir à trouver le *lucidus ordo* que réclamait le poète latin. Qu'on veuille bien m'excuser aussi d'avoir donné souvent une forme tranchante et aphoristique à des propositions dont l'évidence n'est peut-être pas suffisante pour tous. Une démonstration plus complète eût entraîné des longueurs que j'ai tenu à éviter. Ma conviction, trop nettement et peut être trop fortement exprimée, est basée en tout cas sur de mûres réflexions et sur l'expérience de longues années d'étude.

Certes le changement d'orientation introduit dans les sciences naturelles par la théorie transformiste n'enlève rien à la valeur positive des résultats acquis antérieurement par la méthode purement descriptive et nous n'avons pas à faire fi des matériaux lentement accumulés par nos prédécesseurs. Nous pou-

vons continuer à proclamer d'accord en ce point avec Cuvier :

« La détermination précise des espèces et de leurs caractères distinctifs fait la première base sur laquelle toutes les recherches de l'histoire naturelle doivent être fondées ; les observations les plus curieuses, les vues les plus nouvelles perdent presque tout leur mérite quand elles sont dépourvues de cet appui ; et malgré l'aridité de ce genre de travail, c'est par là que doivent commencer tous ceux qui se proposent d'arriver à des résultats solides » (1).

Un grand nombre de naturalistes adonnés à l'étude de la Morphologie systématique ont accueilli avec méfiance l'idée de la variabilité des espèces pensant que cette idée minait les principes sur lesquels reposait leur science de prédilection. Les événements n'ont pas tardé à prouver combien ces craintes étaient chimériques. Pour démontrer scientifiquement la réalité des variations souvent très légères à leur début, il était nécessaire en effet de préciser plus qu'on ne l'avait fait jusqu'alors et de pousser parfois jusqu'à la minutie les descriptions des formes en discussion. La conservation des types dans les collections et les musées, leur représentation graphique et leur comparaison attentive avec les espèces voisines s'imposaient de plus en plus et, certainement, les progrès de l'histoire naturelle systématique ont été fortement stimulés par les contestations des partisans et des adversaires de la théorie de la descendance.

La recherche des formes nouvelles, la poursuite

(1) G. Cuvier *Recherches sur différentes espèces de Crocodiles vivants et sur leurs caractères distinctifs* (*Annales du Muséum*, X, p. 8, 1807).

des types de transition, des aberrations, des variétés locales n'ont plus pour but unique aujourd'hui la satisfaction d'un sentiment de vague curiosité. La connaissance des moindres modifications de structure, des moindres écarts de la morphologie normale deviennent des éléments précieux pour l'établissement des arbres phylogéniques.

La classification naturelle, au lieu d'être une entité subjective variable avec les conceptions particulières de chaque systématiste, se présente désormais à l'esprit comme une réalité objective : l'histoire généalogique des êtres vivants dont on peut concevoir déjà un plan général très imparfait sans doute mais à l'établissement duquel devront concourir toutes les découvertes ultérieures.

Les travaux de spécification ont un but supérieur, un rôle non seulement de description, mais aussi de prévision et partant, ils s'élèvent d'un degré dans l'échelle des connaissances humaines. Leur intérêt devient donc bien plus considérable.

Et cet intérêt n'est pas borné à la science des êtres vivants actuels; il s'étend à la recherche des formes éteintes recélées à l'état de pétrifications dans les entrailles de la terre.

La paléontologie s'ouvre à nous comme un gigantesque dépôt d'archives et, malgré de regrettables lacunes que l'avenir comblera sans doute de plus en plus, elle nous apporte les documents les plus précieux pour retracer la lignée ancestrale des végétaux des animaux et de l'homme lui-même. Véritables *médailles de la création* les fossiles nous permettent de reconstituer sur des bases solides l'histoire naturelle des êtres vivants au sens exact du mot par des procédés analogues à ceux que met en usage l'his-

toire proprement dite telle que la comprennent les sociologistes et les philosophes.

Dès à présent la Paléontologie forme donc avec la Zoologie un tout indissoluble et ces deux parties de la Morphologie se prêtent réciproquement un mutuel concours. Mais la Zoologie est incomplète. Malgré les efforts des générations qui nous ont précédés nous sommes loin de connaître encore tous les êtres vivants actuellement existants à la surface du globe. La Paléontologie ne nous donne que de trop rares indications si l'on songe à la quantité formidable d'organismes disparus sans laisser de traces durables : êtres protoplasmiques dépourvus de squelette ou à squelette peu résistant, etc ; si l'on songe surtout aux conditions difficilement et rarement réalisées qui ont pu assurer la fossilisation et la conservation des animaux à travers toutes les vicissitudes de l'écorce terrestre. Beaucoup de ces lacunes de la série morphologique sont en train de disparaître ou disparaîtront peu à peu grâce aux procédés d'investigation plus puissants dont nous disposons, grâce aussi aux progrès de la géographie physique et à l'étude plus intensive des territoires jusqu'à présent inexplorés.

La Géonémie ou étude de la distribution géographique est, elle aussi, puissamment éclairée et simplifiée par les doctrines transformistes. La répartition actuelle des animaux et des plantes ne doit plus être considérée comme le résultat du hasard ou d'un principe directeur remplaçant les êtres anciens par des créations nouvelles ainsi qu'on voit au théâtre le décor changer à chaque lever du rideau.

Il existe un lien causal entre le passé et le présent. La Paléontologie nous indique les points

du globe où nous devons chercher les formes à caractères archaïques et la Géonémie à son tour en nous faisant deviner les révolutions de l'écorce terrestre nous dévoile les motifs lointains de la suppression des êtres à jamais disparus,

Mais plus encore que la Géonémie, une science nouvelle ou plutôt le développement prodigieusement rapide d'une branche de la science morphologique jusque-là trop négligée devait bientôt remédier à l'insuffisance inévitable des données zoologiques actuelles et de nos connaissances paléontologiques.

Alors que les naturalistes se contentaient de cataloguer et de comparer entre elles à la façon d'un collectionneur d'armes ou d'objets d'art quelconques les formes multiples dont ils admiraient l'étonnante variété, fruit de l'imagination inépuisable d'un créateur infiniment ingénieux, c'était surtout sur les états adultes, seuls considérés comme *parfaits*, que se portait leur attention. Peu leur importait de savoir comment s'étaient constitués les objets de leur passion favorite. A part quelques rares précurseurs (Aristote daus l'antiquité, Malpighi, Swammerdam, Harvey, C. F. Wolff à une époque plus récente) la plupart des biologistes se désintéressaient de l'étude du développement.

Encore aujourd'hui d'ailleurs on retrouve chez bien des systématistes comme une sorte de vestige de cet état d'esprit. Sur mille entomologistes combien en compte-t-on qui attachent le moindre intérêt à la récolte des chenilles ou des larves d'insectes? Sur cent ornithologistes combien daignent admettre dans leurs collections les nids ou les poussins des oiseaux?

Ce n'est pas le moindre service qu'ait rendu à la Biologie la théorie de l'évolution que d'avoir montré l'importance et la nécessité des études embryologiques.

Il est juste de reconnaître que le terrain était préparé par le développement simultané d'autres branches collatérales de la science et en particulier par les progrés de la micrographie et l'avènement de la théorie cellulaire.

Mais on a le droit de proclamer cependant que c'est surtout au désir de vérifier dans une voie nouvelle les idées de Lamarck et de Darwin qu'il faut attribuer l'abondance et la perfection des recherches embryologiques poursuivies à la suite de Jean Müller et de Von Baer par les Gegenbaur, les Haeckel, les Leuckart, les Huxley, les Lœven, les Van Beneden, les Agassiz, etc.

Par sa continuité, par la dépendance de ses phases puccessives, par le nexus causal qui les détermine et les relie entre elles, le développement des larves et des embryons, ou pour parler le langage moderne, la série ontogénique des stades embryonnaires est merveilleusement propre à illustrer par des exemples d'une évidence convaincante la théorie de la descendance modifiée.

Sans doute, même avant la publication des travaux de Darwin et la belle suite de monographies embryogéniques dont nous venons de parler, Serres avait entrevu, par une sorte de divination géniale, l'idée féconde de la répétition à l'état transitoire dans le développement individuel des formes réalisées d'une façon permanente dans la série zoologique actuelle. Mais cette idée ne pouvait être bien comprise et orter tous ses fruits que lorsqu'elle fut complétées

et solidement démontrée par Fritz Müller dans son admirable petit livre *Für Darwin*.

Dès lors le triple parallélisme existant entre la série zoologique, la série ontogénique et la série paléontologique s'imposait comme une conséquence nécessaire de la parenté phylogénique des animaux, et comme la traduction évidente de leurs rapports généalogiques. En outre comme cela doit arriver dans l'application de toute théorie sérieuse, les apparentes exceptions dues aux abréviations ou aux falsifications de l'évolution ontogénique pouvaient être prévues et expliquées en partie par les principes mêmes de la doctrine darwinienne, la sélection naturelle et la concurrence vitale.

C'est donc avec pleine raison que le principe de Serres et de Fritz Müller a été désigné par Hæckel sous le nom de *loi biogénétique fondamentale*, si nous donnons à ce mot *loi* le sens qu'on lui accorde ordinairement dans les sciences expérimentales, celui d'une formule générale susceptible de vérifications suffisantes et nous permettant de prévoir indéfiniment des faits nouveaux.

Riche des travaux de Daubenton, de Haller, de Camper, de Pallas, de Vicq d'Azyr, l'Anatomie comparée semblait avoir reçu du génie de Cuvier ses bases désormais inébranlables.

Elle ne devait pas échapper cependant à l'action rénovatrice des idées évolutionnistes. Les problèmes qu'elle s'était posés de tout temps, les questions auxquelles elle croyait avoir répondu, renaissaient bientôt sous des formes imprévues; Huxley, Gegenbaur, Leuckart ne tardèrent pas à nous montrer dans quelle voie il convenait d'en chercher la solution définitive.

La prétendue loi de la corrélation des formes

(Cuvier), le principe des connexions (Et. Geoffroy Saint-Hilaire), celui du balancement des organes, l'idée de la dégradation des types (de Blainville), la notion d'organes rudimentaires, etc., au lieu d'être de simples formules empiriques, devenaient l'expression synthétique de rapports réels et nécessaires entre les organismes reliés par la consanguinité et si elles n'avaient été établies déjà solidement par voie inductive, ces conceptions auraient pu être déduites comme des corollaires obligatoires de la parenté généalogique des êtres vivants.

Si l'on se reporte aux mémoires de l'époque et à la fameuse discussion entre Cuvier et Et. Geoffroy Saint-Hilaire sur l'unité de composition organique, discussion dont Gœthe suivait avec tant de passion les péripéties qu'il y concentrait les forces de son esprit et se désintéressait de la révolution politique, préoccupation de tous en 1830, on reconnaît avec étonnement que ni l'un ni l'autre des illustres adversaires n'entrevoyaient la signification bien plus haute qu'aurait pris le débat si l'on avait tenu compte des idées que Lamarck soutenait alors depuis près de vingt ans déjà au milieu de l'indifférence générale des naturalistes et des philosophes.

Il ressort en effet de nombreux passages de la *Philosophie anatomique*, que E. Geoffroy Saint-Hilaire lui-même ne voyait dans l'unité du plan d'organisation que l'expression d'une parenté idéale et qu'il cherchait à l'expliquer, comme on a souvent tenté de le faire plus récemment, par une comparaison avec les produits successifs de l'architecture humaine destinés à des usages similaires (1).

(1) Voici quelques lignes de Geoffroy tout à fait significatives à cet égard :

« Des rapports que j'aperçois entre des matériaux, lesquels

Au point de vue philosophique il n'y avait donc pas un abîme entre Cuvier et Geoffroy. Tous deux étaient des créationnistes, mais tandis que Cuvier admettait la pluralité des types (réalisés au nombre de quatre au moins par le créateur), E. Geoffroy Saint-Hilaire considérait le règne animal tout entier comme la manifestation d'une pensée unique développée suivant un plan invariable dans ses grandes lignes, modifiable seulement dans les détails.

On comprend sans qu'il soit nécessaire d'insister davantage quelle lumière apportait dans cette question des plans d'organisation la théorie de la des-

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reviennent les mêmes pour composer les animaux, de ces données qui produisent une certaine ressemblance chez tous les êtres, tant à l'intérieur qu'à l'extérieur, j'arrive à une déduction, à une idée générale qui comprend toutes ces coïncidences; et si je les embrasse et les exprime sous la forme et le nom *d'unité d'organisation*, je ne me propose par là que de traduire ma pensée en un langage simple et précis; mais d'ailleurs je me garde bien de dire ce que j'ignore, qu'une chose serait faite avec intention à cause d'une autre? En définitive, je me crois, dans ces conclusions, aussi fondé en raison que si, voyant d'ensemble les nombreux édifices d'une grande ville et me restreignant aux points communs que *leur imposent* les conditions de leurs existences, j'en venais à réfléchir sur les principes de l'art architectural, sur l'uniformité de structure et d'emploi d'un autre grand nombre d'édifices. Une maison n'est point faite en vue d'une autre; mais toutes peuvent être ramenées intellectuellement à l'unité de composition, chacune étant le produit de matériaux identiques, fer, bois, plâtre... de même qu'à l'unité de fonctions, puisque l'objet de toutes est également de servir d'habitation aux hommes... »

Et plus loin :

« Toute composition organique est la répétition d'une autre, sans être de fait produite par le développement et les transformations successives d'un même noyau. Ainsi il n'arrive à personne de croire qu'un palais ait d'abord été une humble cabane qu'on aurait étendue pour en faire une maison, puis un hôtel, puis un édifice royal. (Et. Geoffroy Saint-Hilaire. *Philosophie anatomique*).

cendance modifiée et l'étude des adaptations progressives des êtres vivants aux conditions d'existence variables suivant le temps et les lieux.

On comprend aussi le sens précis et profond que revêtaient les notions jusqu'alors assez vagues d'*Analogie* et d'*Homologie*; celles plus récentes d'*Homomorphie* et d'*Homophylie*, etc.

La *convergence* des formes sous l'influence des facteurs éthologiques (vie pélagique, vie parasitaire, etc.) cessait de masquer les véritables affinités et peu à peu disparaissaient de la systématique épurée les groupements factices introduits par ce qu'on pourrait appeler les *idola ethologica*.

Plus difficiles à éliminer furent les *idola tectologica*. La notion des types organiques, si importante comme nous venons de le voir, a été longtemps obscurcie par l'imprécision de nos connaissances sur l'individualité ou plutôt sur les individualités de divers ordres. Chez les animaux composés, en particulier, tels que les Spongiaires, les Hydraires, les Bryozoaires, les Synascidies, on a longtemps attribué une valeur taxonomique fort exagérée à la cormogenèse c'est-à-dire au mode de groupement des individus, en négligeant les rapports réels de parenté que révèle la structure anatomique de ces individus. Ce n'est pas un des moindres services rendus par E. Hæckel à la science biologique que d'avoir tenté le premier de fixer les règles de cette branche de la morphologie qui est comme *l'Architectonique* des êtres vivants et qu'il a dénommée tectologie.

Chez les Métazoaires en particulier la notion tectologique de la *personne*, c'est-à-dire de l'être originairement diblastique (*gastrula*) qui constitue le

mode d'individualité le plus fréquent est une acquisition d'une valeur inappréciable.

Entrevue par de Blainville et par Huxley, qui la déduisaient de considérations purement anatomiques, cette notion fut nettement établie par Hæckel dès 1872 grâce surtout aux admirables recherches embryologiques d'Alexandre Kowalevsky, recherches qui prouvaient l'existence de la larve gastrula chez tous les groupes d'animaux pluricellulaires où le développement est explicite.

Malgré les attaques récentes qu'elle a subies, la théorie de la gastrula bien comprise est demeurée aussi inébranlable que celle de l'homologie des feuillets blastodermiques qui en est la conséquence immédiate.

L'application raisonnée du principe de Fritz Müller suffit à rendre compte des difficultés que présentent certains développements condensés ou cœnogénétiques et les objections présentées par quelques auteurs tiennent souvent à ce qu'ils n'ont étudié qu'un trop petit nombre de types (parfois un type unique) choisis plutôt en raison de la commodité pratique et sans tenir compte des perturbations des facteurs éthologiques auxquelles ces types étaient soumis.

L'idée d'une forme originelle commune mais modifiée souvent d'une façon profonde par l'action des milieux, faisait aussi ressortir l'inanité des rapprochements basés uniquement sur la Promorphologie cette sorte de cristallographie ou de géométrie des êtres vivants.

Des groupements tels que ceux des *Rayonnés* ou *Radiata*, des *Bilateralia*, etc., sont purement artifi-

ciels et inspirés seulement par les *idola promorphologica*.

Il n'en demeure pas moins qu'il serait très désirable de pousser plus loin qu'on ne l'a fait jusqu'à présent les études promorphologiques dont Hæckel a jeté les bases dans son admirable *Generelle Morphologie*. En ce point comme en beaucoup d'autres la Morphologie est étroitement dépendante de la Géométrie et de la Mécanique. Il y a là matière à de nombreux problèmes d'un intérêt très vif pour qui ne veut pas se contenter de la facile mais enfantine solution des causes finales.

« Voir venir les choses, avait dit Savigny, est la meilleure façon de les observer. » La Morphologie en éclairant d'une vive lumière l'Anatomie comparée permet de rectifier de nombreuses erreurs de Systématique et de mieux apprécier la valeur des divers groupements taxonomiques. Mais en même temps qu'elles servaient au progrès de la Morphologie normale, les études embryogéniques étendues aux formes anormales de développement montrèrent l'intérêt puissant de la science des monstruosités ou Teratologie. Bientôt, grâce aux patientes investigations de Dareste et à l'habileté de Chabry dans la production artificielle des êtres animaux, la Tératogénie devint une science expérimentale et il fut dès lors facile de comprendre comment en intervenant d'une façon plus ou moins constante aux diverses périodes de l'ontogénie, les facteurs cosmiques ou biologiques ont pu modifier graduellement les formes larvaires et indirectement les formes adultes des êtres vivants.

Par suite la science des mœurs et des rapports des

êtres vivants soit entre eux, soit avec le milieu cosmique, l'Ethologie ou la Bionomie, un peu négligée depuis l'époque où Réaumur, de Geer, etc., la cultivaient avec tant de succès, prenait un intérêt nouveau et offrait au biologiste toute une collection d'expériences préparées par la nature et dont il n'avait plus qu'à interprèter les résultats.

N'est-il pas remarquable en effet de voir l'Ethologie de l'adulte modifier d'une façon profonde le développement de l'embryon au point de masquer parfois au cours de l'évolution les affinités qui existent entre des formes voisines?

Le régime herbivore ou carnassier d'un Mammifère par exemple n'entraîne-t-il pas comme conséquence l'état de perfection du premier à la naissance et par suite l'abréviation des processus embryogéniques, le petit ne pouvant être protégé suffisamment par ses parents dont il doit en outre suivre les déplacements rapides pour la recherche de la nourriture ou pour la fuite devant l'ennemi.

Les animaux fixés à l'état adulte et surtout les parasites établis de bonne heure sur l'hôte qu'ils ne quittent plus ont nécessairement un développement explicite et des larves mobiles pourvues d'organes des sens qui leur permettent de choisir avec soin la demeure où s'écoulera la plus grande partie de leur existence. Au contraire pour les êtres pélagiques qui, dans le jeune âge sont exposés à mille dangers, il y aura tout intérêt à ce que leur progéniture soit protégée par un développement direct rapide et cœnogénétique ou soit confiée à une nourrice étrangère comme c'est le cas pour les Copépodes du groupe des *Monstrillidæ*.

Même des phénomènes évolutifs aussi compliqués

que ceux connus chez les Coléoptères Méloïdes sous le nom d'hypermétamorphoses, deviennent très facilement explicables si on les envisage dans leurs rapports avec les conditions éthologiques et comme une conséquence obligatoire de la vie qu'ont dû mener les ancêtres.

Non moins intéressantes, il me semble, les particularités embryogéniques que j'ai réuniessous le nom de *Pœcilogonie*. Deux êtres appartenant à la même espèce aussi semblables quo possible à l'état adulte, tellement semblables parfois que l'œil du spécialiste le plus exercé ne réussira pas à trouver entre eux la moindre différence, peuvent présenter dans la série de leurs stades ontogéniques et même sous la forme ovulaire, des divergences très accentuées si leur éthologie embryonnaire n'est pas la même, si par exemple le milieu n'a pas la même composition chimique ou si la saison du développement est différente, ou encore si les conditions biologiques varient avec l'ambiance cosmique dans les divers habitats d'une espèce à vaste dispersion. D'où les dénominations de *pœcilogonie géographique*, *pœcilogonie saisonnière*, etc.

Quoi de plus étonnant aussi que ces curieuses expériences de Morphogénie réalisées par la nature et que j'ai fait connaître autrefois sous le nom de *castration parasitaire* ? Et quelque mystérieuse que soit pour nous l'action modificatrice du parasite gonotome indirect, n'est-il pas très instructif aupoint de vue morphodynamique de voir ce parasite faire apparaître par une action à distance sur un hôte d'un sexe déterminé les caractères du sexe opposé alors même que ces caractères n'auront pour l'être qui les porte aucune utilité ? Enfin cette notion d'un

complexe morphologique formé par l'hôte et son parasite ne prend-elle pas une importance capitale lorsqu'on rapproche ces complexes parasitaires à équilibre biologique instable des complexes homophysaires ou hétérophysaires à équilibre plus ou moins permanent réalisés soit chez les galles (cécidies ou thylacies), soit chez les êtres symbiotes tels que les Lichens, les plantes à mycorhizes etc.

Au surplus la notion des complexes d'êtres divers associés en symbiose harmonique n'est qu'une généralisation de ce que nous observons chez tous les organismes pluricellulaires au cours de leur évolution.

Dès le milieu du xviii^e siecle C. Fréd. Wolff a établi sur des bases inébranlables la théorie de l'épigénèse. Il a montré que les êtres vivants ne se développent pas comme on l'aurait cru aux dépens d'un rudiment préformé, en grandissant à peu près comme grossit l'image d'un objet examiné successivement avec des lentilles de puissances graduellement croissantes.

Les divers organes d'un animal sont des formations d'une autonomie relative qui concourent à édifier un ensemble dont l'équilibre n'est pas préétabli et dont le plan peut parfois être modifié en voie de construction.

Bien entendu les liens de dépendance réciproque des divers systèmes d'organes varient beaucoup. Parfois très étroite quand les fonctions que doivent remplir les organes sont elles-mêmes étroitement liées (respiration et circulation par exemple) ils peuvent être beaucoup plus lâches lorsqu'il s'agit de parties adaptées à des rôles très distincts (organes de

locomotion et appareil digestif, ou bien système tégumentaire et squelette, etc).

Mais l'indépendance est surtout très grande si l'on considère d'une part les organes qui servent à la vie de l'individu et, d'autre part, ceux qui sont destinés à assurer la perpétuation de l'espèce.

Le *soma* et les *gonades* pour employer les expressions modernes qui désignent ces deux ensembles sont en quelque sorte deux organismes juxtaposés ou emboîtés l'un dans l'autre dont le développement peut marcher d'un pas très inégal, bien que toute modification apportée à l'un d'eux ait en général un retentissement sur l'autre.

C'est en s'appuyant sur cette notion fondamentalement exacte mais en l'exagérant et en l'enveloppant d'une atmosphère métaphysique que les partisans de l'ancienne théorie de l'évolution (préexistence et emboîtement des germes, préformation de l'embryon) ont longtemps lutté contre les idées de C. F. Wolff.

C'est encore en suivant le même courant d'idées que plus récemment A. Weismann a cherché à édifier ses théories bien connues sur la non-transmissibilité des caractères acquis.

C'est enfin la même considération qui, étendue aux premières phases de l'embryogénie, aux diverses cellules de la morula et même aux diverses régions de l'œuf non encore segmenté a servi de base à la théorie de la mosaïque de W. Roux si ingénieusement modifiée depuis par E. B. Wilson.

En nous en tenant à la stricte observation des faits les plus faciles à vérifier nous dirons seulement que l'épigénèse, en nous révélant la possibilité d'une

sorte de concurrence vitale entre les organes et même entre les plastides constituant les êtres pluricellulaires, nous permet d'expliquer facilement tous les faits si complexes de polymorphisme évolutif ; la progénèse, la néoténie, la dissogonie, la pœcilogonie et en général les particularités si curieuses de développement que depuis Chamisso et Steenstrup on a groupé sous la nom très impropre de générations alternantes, ou de généagenèse (de Quatrefages).

Il s'est constitué ainsi un vaste ensemble de connaissances assez étendues pour constituer aujourd'hui une branche nouvelle de la Morphologie qu'on pourrait appeler la *Génésiologie*.

La Génésiologie a pour objet l'étude à la fois descriptive et expérimentale des divers modes évolutifs.

Dans les pages précédentes nous avons à diverses reprises parlé de l'expérience et de la méthode exrimentale dans un sens différent de celui qui est souvent donné à ces mots par les physiologistes de l'ancienne école.

C'est peut-être ici le lieu d'indiquer la façon dont nous comprenons l'intervention de l'expérience dans les sciences morphologiques et le résultat qu'on peut en attendre pour le développement ultérieur de ces sciences.

Une expérience nécessite toujours l'analyse préalable des phénomènes par lesquels est conditionné le fait que l'on veut observer et, si possible, mesurer. Elle suppose un déterminisme hypothétique dont elle démontrera la réalité ou la non-existence. Toute expérience est donc précédée d'une induction et par suite d'une ou plusieurs observations. La mé-

thode expérimentale est toujours, comme le disait Chevreul, une méthode *à posteriori*.

L'expérience ne crée rien; elle a tout juste la même valeur et la même signification logique que la preuve d'une opération arithmétique.

Pour qu'il y ait expérience, il n'est donc pas nécessaire d'exiger comme certains paraissent le croire un dispositif compliqué, un laboratoire richement outillé et des appareils coûteux.

Il ne faut pas confondre en effet la mesure précise d'un phénomène qui n'est souvent obtenue qu'à l'aide d'instruments très délicats et la constatation pure et simple d'une relation de causalité entre un fait et d'autres faits qui le déterminent, constatation qui est le fond même de l'expérience. Le fait fût-il accidentel, comme la chute de la pomme sous les yeux de Newton, sa constatation peut devenir néanmoins une expérience. Et c'est l'esprit seul de l'observateur qui lui donnera ce caractère.

> Das ist ja was den Menschen zieret
> Und dazu ward ihm der Verstand
> Dasz im innern Herzen spüret
> Was er erschafft mit seiner Hand.

Là où le vulgaire voit sans interpréter et garde une attitude purement contemplative, le naturaliste digne de ce nom remplace par la supposition d'actes volontaires les facteurs dont il veut étudier l'activité.

Un animal reçoit à la chasse ou par tout autre accident une balle dans la partie gauche du cerveau, il est paralysé du côté droit. Si le fait est bien constaté et dégagé de toute cause d'erreur, sa reproduction volontaire dans un laboratoire ne sera que la vérification d'une expérience déjà réalisée.

Non seulement la nature actuelle nous offre, comme nous l'avons dit, de très nombreuses expériences dont beaucoup sont même très difficiles à répéter, mais nous pouvons dire aussi que la Paléontologie nous procure des données expérimentales d'une valeur inappréciable. Les arguments qu'elle fournit à la Morphologie transformiste ne sont pas, comme on le prétend parfois, d'ordre purement conjectural ; le degré de certitude qu'ils comportent n'est nullement inférieur à celui qu'on obtient en Astronomie ou dans les autres parties des sciences physiques dont les objets nous sont en partie inaccessibles.

Hilgendorf et Hyatt étudient les diverses couches du lac Tertiaire de Steinheim en Wurtemberg. Ils reconnaissent que certaines formes de *Planorbis* peu différentes entre elles dans les couches profondes (les plus anciennes) (1) se séparent peu à peu les unes des autres et arrivent à constituer dans les couches les plus récentes des espèces aussi valables que toutes celles décrites dans ce genre de Mollusques (2). Est-ce là un travail de pure contemplation et de description ? N'est-il pas manifeste que les auteurs ont dans leur pensée reconstitué une gigantesque expérience. Et s'ils n'avaient en leur pouvoir le déterminisme complet de cette expérience, au moins en possédaient-ils des éléments suffisants pour conclure à l'évolution des formes sans préciser

(1) Les quatre formes les plus anciennes seraient des variétés légères d'une même espèce : *Planorbis lævis.*

(2) Voir : The genesis of the tertiary species of *Planorbis* at Steinheim (*Boston Soc. Nat. Hist.* 1880) et : Transformations of *Planorbis* at Steinheim (*American Naturalist*, 1882, p. 441). Cf. aussi Stearns (*Proceed. Acad. Nat. Sc. Philadelphia*, 1881.

les facteurs de cette évolution autres que le facteur *temps* dont l'action est irrécusable en cette circonstance ?

Bien plus nette et plus évidente encore et en tous cas plus conforme aux idées courantes est l'application de l'expérience à l'étude des facteurs lamarckiens ou facteurs primaires de l'évolution (facteurs cosmiques, éthologiques, etc.) (1).

Et en effet, c'est surtout par un retour aux idées de Lamarck que le transformisme devait faire progresser plus rapidement la morphologie dans la voie expérimentale.

Certes les conceptions de Darwin étaient à bien des égards justiciables de l'expérience, même au sens le plus strict du mot, et Darwin l'a prouvé lui-même par ses belles recherches sur la fécondation directe et croisée, sur les plantes volubiles et les plantes carnivores, etc. Mais il faut le reconnaître, beaucoup de vérifications expérimentales relatives à la sélection naturelle, à l'hérédité, exigent des conditions rarement réalisées, une longue durée qui les rend abordables seulement par des êtres collectifs (sociétés savantes) ou nécessitent des ressources considérables dont ne peuvent disposer le plus grand nombre des travailleurs.

A part quelques brillantes exceptions sur lesquelles nous aurons occasion de revenir, les disciples de Darwin qui ont suivi de plus près les tendances du maître ont surtout compris l'expérience

(1) Il suffit pour s'en convaincre de parcourir notamment les deux beaux volumes publiés récemment par C. B. Davenport sous le titre, *Experimental morphology* (N. York, 1897-99) où l'on trouvera un excellent résumé de tout ce qui a été tenté jusqu'ici dans l'étude des facteurs primaires.

dans le sens très large que nous attribuons à ce mot appliqué à un grand nombre de recherches relatives aux facteurs secondaires.

L'importance de l'étude des facteurs primaires de l'évolution n'avait pu échapper à Darwin. Mais excellent observateur comme il l'était, il fut sans doute effrayé par la complexité du rôle de ces facteurs et n'essaya pas de débrouiller les mécanismes qui donnent naissance aux innombrables variations des êtres vivants.

Ces variations existent, il les signale et sans les rattacher à leurs causes immédiates, il cherche à démontrer avant tout qu'elles ont pu être fixées pour constituer les races, puis les espèces nouvelles.

Darwin avait lu Malthus : il connaissait la loi de la division du travail empruntée par H. Milne-Edwards à l'économie politique ; il trouva que la méthode des sociologistes était bonne et que dans une science compliquée et jeune encore, telle que la Biologie, on pourrait employer les procédés, usités également en Météorologie, en Statistique, etc., s'appuyer sur la loi des grands nombres sans trop chercher à démêler les causes lointaines et à pénétrer l'essence des phénomènes.

C'est ainsi qu'il démontre l'importance de la sélection pour la fixation des caractères acquis lorsqu'ils présentent quelque utilité dans la concurrence vitale et assurent par cela même la survivance de leur possesseur par une meilleure adaptation.

Mais il ne cherche pas à établir dans chaque cas particulier le déterminisme exact de l'apparition des variétés indifférentes ou avantageuses. Peut-être aussi fut-il détourné de cette voie par l'insuccès de son génial prédécesseur Lamarck dans l'effort éner-

gique qu'avait tenté celui-ci pour expliquer par les milieux ambiants (agissant directement ou indirectement par la création de nouveaux besoins) les modifications graduelles des êtres vivants et la transformation des espèces.

Il ne faut pas oublier non plus qu'au début du XIX^{e} siècle et même au moment où parut *l'Origine des espèces*, l'état des sciences physicochimiques ne permettait pas d'aborder utilement la plupart des problèmes de physiologie externe dont il eût été important de chercher la solution : les recherches chimiques déterminant les variations de couleur, l'influence des divers ordres de radiations, l'action morphogène des solutions salines, de l'osmose, etc.

Quelques satisfaisantes qu'elles fussent pour l'esprit et malgré les énormes progrès qu'elles ont fait faire à la Morphologie, les idées de Darwin ne tardèrent donc pas à paraître insuffisantes. On put même croire un moment que les exagérations de quelques-uns des disciples du maître ne compromît le triomphe de la doctrine et ne ramenât les esprits vers les anciennes explications finalistes savamment ressuscitées sous le nom de Néovitalisme. Les mots sélection naturelle, mimétisme, convergence, hérédité et autres semblables qui, dans la pensée de Darwin, n'avaient qu'une valeur explicative *provisoire*, devinrent pour les philosophes et même pour certains biologistes des formules commodes servant à masquer l'ignorance dans laquelle nous sommes le plus souvent à l'égard des causes prochaines de variation.

Et cependant lorsqu'en 1880 il publia son petit livre si suggestif, *Die Existenzbedingungen der Thiere* Carl Semper s'efforçait déjà de ramener les natura-

listes vers l'étude des facteurs primaires. Aux expériences de rares précurseurs sur l'influence morphogène du changement de régime alimentaire (Hunter Edmondstone) des modifications de saluro de l'eau (Smankevitch), de la chaleur de la lumière etc., il ajoutait ses recherches originales sur les conditions optima de la croissance et de la reproduction des Lymnées et surtout il réunissait sous un volume restreint mais avec une documentation très complète pour l'époque, une masse énorme d'observations biologiques dont un grand nombre ont absolument la même valeur démonstrative que les meilleures expériences de laboratoire. Depuis, les recherches de ce genre ont été reprises avec ardeur de différents côtés et surtout en Amérique. L'impulsion est donnée et l'on peut être assuré que le mouvement ira en s'accentuant à moins que les progrès parallèles de la Physique et de la Chimie permettent d'apporter dans ces études une précision plus grande et d'aborder certaines questions qui, jusqu'à présent, semblaient inaccessibles.

Le défrichement de territoires scientifiques nouveaux tels que la Physicochimie et la Chimie biologique nous fournira bientôt les moyens de reprendre utilement l'œuvre dont Lamarck n'avait pu que tracer les grandes lignes.

La dépendance de la Morphologie par rapport aux sciences physicochimiques est encore plus manifeste dans cette branche si neuve et si pleine de promesses qu'on désigne sous le nom de Cytologie.

Bien que la théorie cellulaire, déjà ébauchée par Malpighi, ait été complètement formulée par Raspail (1835) et par Schleiden (1838) pour les végétaux, puis par Schwann (1839) pour les animaux, bien que

Virchow eût dès le milieu du siècle dernier proclamé son célèbre aphorisme *omnis cellula e cellula*, c'est seulement dans le cours de ces vingt dernières années que la Morphologie cellulaire et la Cytogénie ont pris un merveilleux développement grâce aux travaux de Ed. Van Beneden, de Strasburger, et de toute une brillante phalange de jeunes biologistes.

L'histoire de ce magnifique édifice, son plan général et ses détails ont été très exactement retracés dans un ouvrage désormais classique, *Cell and inheritance*, publié dès 1896 par E. B. Wilson, un des vaillants ouvriers qui avec O et R. Hertwig, Boveri, Maupas, Guignard etc, ont le plus activement contribué à son édification. Mais combien laborieusement cette œuvre difficile a-t-elle été préparée par les nombreux perfectionnements de la technique microscopique dus aux Leydig, aux Ranvier, aux Max Schultze, aux Flemming, etc.

Et ces perfectionnements à leur tour n'ont pu être obtenus que grâce aux progrès de la Chimie et particulièrement de la Chimie des colorants (des couleurs d'aniline en particulier). Malgré la façon empirique et grossière dont nous utilisons chaque conquête nouvelle des sciences physicochimiques, malgré ce qu'ont encore d'imparfait des méthodes telles que celles de Golgi, de Cajal et d'Apathy, quel morphologiste serait assez aveugle pour nier l'importance des données nouvelles que nous devons à des procédés techniques dont la théorie nous est très souvent inconnue?

Mais la chimie nous a rendu des services non moins éminents en nous permettant de pénétrer dans la structure intime de la substance chromatique et des albuminoïdes en général. Dans cette voie fé-

conde, qui avait déjà tenté Robin, mais qui nous a été ouverte par Schützenberger et par Kossel, la Morphologie cytologique trouvera à coup sûr la clef de bien des énigmes qui l'arrêtent à l'heure actuelle. Et quels progrès ne devons-nous pas attendre de la Chimie des colloïdes dont notre Chimie actuelle n'est en quelque sorte qu'un cas particulier?

Que la Morphologie cytologique soit tributaire également et dans une large mesure de la Physique et surtout de l'Optique, cela est trop évident pour qu'il soit nécessaire d'y insister longuement, Je voudrais seulement faire incidemment une remarque qui montre bien quelles répercussions peuvent avoir les unes sur les autres des études scientifiques en apparence très dissemblables.

Il n'est pas douteux que le perfectionnement des appareils micrographiques et surtout des objectifs à immersion a été dû pour une bonne part au désir qu'avaient les constructeurs de satisfaire une clientèle spéciale et assez nombreuse dans certains pays les collectionneurs de Diatomées, de telle sorte que ces amateurs parfois injustement dédaignés par ceux qui veulent établir des cloisons étanches entre les savants de divers ordres ont rendu indirectement de grands services aux histologistes purs et à ceux qui étudient les problèmes les plus délicats de la Cytologie et de la Cytogénie.

Les bactériologistes tout en visant un but bien différent et beaucoup plus pratique ont concouru plus encore que les diatomistes au perfectionnement de notre outillage micrographique en étendant à toute une classe nouvelle de travailleurs, les pathologistes et les cliniciens l'usage quotidien du microscope.

Et dans ce domaine de l'Anatomie pathologique nous voyons encore se produire ces intéractions si fructueuses avec la science qui nous occupe plus spécialement. L'étude des tumeurs, la Tératologie cellulaire, en même temps qu'elle s'éclaire par les données de la Cytologie normale, nous fournit des aperçus bien suggestifs sur la signification de la réduction chromatique et des rapprochements inattendus avec les phénomènes si nouveaux des premières phases embryogéniques (Borrel, Moore et Farmer).

L'idée de la phagocytose puisée par Hæckel dans la biologie des Protozoaires, par Rouget dans l'examen des leucocytes du sang, mise en valeur et magistralement développée par Metchnikoff qui en fit d'innombrables applications dans le domaine de la Pathologie, est revenue par un retour des plus heureux élucider certains phénomènes morphologiques les plus obscurs de l'Embryogénie, les processus cœnogénétiques de l'ovogénèse et de la métamorphose.

Longtemps l'introduction des sciences mathématiques dans le domaine de la Morphologie a été tenue pour suspecte ; il paraissait dangereux en effet de vouloir enchaîner par des formules trop simples des faits aussi complexes que ceux étudiés par les zoologistes et les botanistes.

Peu à peu cependant la nécessité se fit sentir de déterminer par des mesures précises l'étendue des variations dues aux facteurs primaires et de chercher à trouver les lois de ces variations. Un des premiers, Delbœuf essaya non sans succès l'application de l'algèbre au problème de la formation des races. Mais c'est surtout à Galton et à son école, que sont

dus les travaux les plus importants de biologie mathématique ou biométrique.

Quelque soit le caractère sur lequel nous portons notre attention, si nous considérons un grand nombre de spécimens d'une espèce déterminée, nous reconnaissons que les variations individuelles (variations continues ou fluctuations) de ce caractère, évaluées numériquement, ne dépassent pas deux limites extrêmes réalisées chez un très petit nombre d'individus. Entre ces deux extrêmes se trouve une variation moyenne constatée chez le plus grand nombre des spécimens observés. Il en résulte que si on prend pour abscisses des lignes représentant l'étendue des fluctuations et pour ordonnées les grandeurs correspondant au nombre d'individus présentant une certaine fluctuation, on obtient une courbe que Quételet appelait une *binomiale* et qui n'est en réalité qu'une courbe d'erreur probable. On donne souvent aussi à ces courbes le nom de courbes de Galton en raison de l'emploi très étendu qu'en a fait cet éminent biologiste dans l'étude des questions d'hérédité.

Par sélection artificielle, les éleveurs et les horticulteurs arrivent à déplacer plus ou moins rapidement les sommets des courbes galtoniennes et à diriger la fluctuation dans le sens qui leur convient. La sélection naturelle n'opère pas autrement pour modifier la morphologie des espèces et c'est à cette action que Darwin attribuait en grande partie la transformation des espèces.

Wallace, plus exclusif, considère la sélection comme le seul facteur déterminant l'évolution des êtres vivants.

Il était réservé à Hugo de Vries de montrer par de

longues et délicates expériences culturales l'exagération dans laquelle étaient tombés les disciples outranciers de Darwin (Romanes, Weismann) (1). Guidé par ses travaux antérieurs sur les courbes galtonniennes et frappé de la constance de certaines formes telles que les espèces décrites par le botaniste Jordan, dont la naissance pouvait difficilement s'expliquer par des fluctuations, de Vries suppose qu'après des périodes de fixité relative pendant lesquelles ils sont soumis seulement à la variation fluctuante, les êtres vivants peuvent traverser des périodes plus courtes où leurs formes sont brusquement modifiées dans des directions diverses par des changements discontinus.

Les biologistes connaissaient bien ce genre de variations qu'ils appelaient *variations sportives*. De Vries les a nommées *mutations* et il a montré l'importance de la mutabilité en étudiant plus spécialement une plante bisannuelle, l'*Œnothera Lamarckiana*, espèce américaine introduite en Europe et subspontanée dans plusieurs localités des Pays-Bas. De 1880 à 1899 de Vries a semé tous les ans au jardin botanique d'Amsterdam jusqu'à 15 ou 20.000 graines de cette plante. A côté de milliers d'individus normaux, ses cultures ont produit *sept* types nouveaux représentés chaque année par un nombre variable d'individus et susceptibles de se reproduire par graine avec une grande fixité. Sur les 50.000 *Œnothera* qu'il a observés pendant dix ans, de Vries en a compté 800 qu'on ne pouvait légitimement désigner sous le nom d'*Œnothera Lamarckiana*, mais qui se

(1) H. de Vries. *Die Mutationstheorie* Leipzig, 1901-1903.

répartissaient, comme nous venons de le dire, en sept groupes auxquels on était en droit de donner la valeur systématique de sous-espèces, ce que les botanistes n'eussent pas manqué de faire si ces plantes avaient été rencontrées dans les champs sans qu'on en connût l'origine.

Un grand nombre de biologistes ont cru trouver dans les beaux travaux de Vries des armes redoutables contre la théorie de la sélection. Il m'est impossible de partager leur opinion. Je dirai même qu'en examinant la question de très près et en pénétrant au fond des choses, je ne puis trouver dans la théorie des mutations autre chose qu'un utile complément des doctrines lamarckiennes et darwiniennes de la variation continue.

Comme le disait l'économiste Bastiat, dans tout phénomène complexe où interviennent en sens divers des causes multiples, il y a ce qu'on voit et il y a aussi ce qu'on ne voit pas.

Ce que l'on voit dans une mutation c'est l'apparition brusque et soudaine d'un caractère qui n'existait pas antérieurement, mais ce caractère n'est que la manifestation subite d'un état qui a pu être très lentement préparé chez les ancêtres de l'individu où il se produit. Pour obtenir une réaction chimique, pour faire virer la coloration d'un liquide, il faut souvent ajouter goutte à goutte le réactif, jusqu'au moment où, tout à coup, la réaction se produit et la coloration nouvelle apparait. La mutation est le résultat d'un nouvel état d'équilibre dans l'organisme en variation. Tous les individus chez lesquels cet équilibre nouveau se prépare sont intérieurement dans un état différent de celui de leurs ancêtres, ils sont

en fluctuation interne et c'est là ce qu'on ne voit pas (1).

Si des modifications doivent se produire dans la nervation des ailes d'un Insecte par exemple, il est impossible que ces modifications se traduisent autrement que par un nouveau dispositif mécanique constituant, par rapport au précédent, une variation brusque de l'agencement des cellules et des nervures. De même, l'apparition d'une vertèbre nouvelle ou d'un métamère nouveau chez un animal dont la métamérisation était fixée, ne peut se faire que d'une façon discontinue et non par fraction infinitésimale de vertèbre ou de métamère. Le fait que les variations sportives apparaissent toujours en nombre limité (7 dans le cas d'*Œnothera Lamarckiana*) montre bien qu'il s'agit d'un certain nombre de positions d'équilibre entre lesquelles il n'y a pas de transitions morphologiques réalisables et dont quelques-unes mêmes semblent difficiles à obtenir. Des 7 sous-

(1) Un botaniste dont les recherches originales sur la variation chez les végétaux n'ont pas assez attiré l'attention, A. T. Carrière, fait à ce propos une ingénieuse comparaison : « Nous pouvons, dit-il, afin de nous représenter le double effet, l'effet lent et l'effet brusque sous lequel se montre le dimorphisme (ce que nous appellerions aujourd'hui une mutation ditaxique) supposer une horloge à secondes dont on ne verrait que le cadran. Dans ce cas, l'effet continu mais lent, nous serait représenté par le balancier, qui, bien que nous ne le voyions pas, ne s'arrête cependant jamais, et l'effet brusque ou intermittent par chaque saut que feraient les aiguilles, saut qui est la résultante d'une action incessante tellement lente qu'elle n'est point appréciable à nos sens et qui ne se manifeste d'une manière sensible que lorsqu'il y a une certaine quantité de force accumulée. » A. Carrière, Production et fixation des variétés dans les végétaux. » (*Revue horticole*, Paris 1868, note 42, p. 71.)

espèces d'*Œnothera*, une seule l'*Œnothera gigas* s'est montrée robuste. Les autres sont pour la plupart très faibles et ont besoin de beaucoup de soin pour fleurir et pour mûrir leurs graines. Souvent même il n'y a que deux équilibres possibles, c'est ce qui a lieu dans les cas de dimorphisme ou de *ditaxies* des couleurs, pour employer le langage de Coutagne, si fréquents chez les végétaux, chez les Mollusques, les Lépidoptères, etc.

En réalité, comme je l'écrivais il y a une dizaine d'années, tandis que les fluctuations peuvent être comparées à des mouvements graduels d'oscillation de part et d'autre d'une position moyenne, les mutations représentent autant d'états d'équilibre stable entre lesquels ne peuvent s'établir des passages continus. Les formes intermédiaires à ces états d'équilibre ne sont pas réalisées explicitement parce qu'elles ne correspondent pas à des états de stabilité suffisante. Pour me servir d'une comparaison triviale qui fera mieux comprendre ma pensée, *on ne peut monter la moitié ou une fraction quelconque d'une marche d'escalier*. Dans des cas semblables le progrès est forcément discontinu, ou, ce qui revient au même, ne se manifeste que d'une façon discontinue. Mais on ne peut tirer de ces faits aucun argument contre la formation des espèces par sélection naturelle; à plus forte raison ne faut-il pas y chercher la solution unique et complète des problèmes si complexes de transformisme (1).

D'ailleurs, de même que Darwin n'a jamais nié l'existence et l'importance des mutations qu'il appe-

(1) A. Giard. Sur un exemplaire de *Pterodela pedicularia*, *L.* à nervation doublement anormale. *Actes de la Soc. Scient. du Chili*, I. V. 1895, p. 21.

lait variations singulières (*single variations*), de son côté de Vries n'a jamais cherché à ruiner la théorie de la sélection.

Au lieu d'opérer seulement sur les individus en fluctuation, celle-ci opère sur les espèces naissantes, la concurrence vitale s'exerçant entre les mutations et les formes d'où elles procèdent. Comme le fait observer très justement W. Hubrecht dans l'analyse si claire qu'il a donnée récemment des idées de son compatriote : « *Far from having undermined Darwin's Darwinism, de Vries has completed, purified and simplified it* » et ceux-là seuls pensent autrement qui combattent le Darwinisme pour d'autres raisons que des raisons scientifiques et souhaiteraient au fond de leur cœur autant de mal aux démonstrations de de Vries et à toutes les autres formes possibles de la théorie de l'évolution (1).

Une autre application intéressante des mathématiques aux sciences morphologiques se présente dans l'étude des formes hybrides. Les lois de Mendel, récemment vérifiées par de Vries, Tschermak, Bateson, etc., relèvent en dernier ressort du calcul des probabilités. Il serait hors de propos d'insister plus longuement sur les problèmes nombreux et

(1) I have purposely insisted on these points, because here and there a tendency seems to prevail to look upon Darwin's views on the origin of species as unsatisfactory and obsolete and to proclaim the necessity of replacing them by a broad new hypothesis with which the name of de Vries should be coupled. These tendencies are in great favor with those that bear a grudge to the so-called Darwinism for other than scientific reasons, and who in their innermost heart would at the same time like to see a similar fate reserved for de Vries's demonstrations and even for the whole theory of evolution. » A. A. W. Hubrecht. Hugo De Vries's theory of mutations. (*The Popular Science Monthly*, July 1904, p. 212.)

importants relatifs à l'hérédité morphologique, dont la solution dépend de l'étude raisonnée de données numériques aussi nombreuses que possible.

De l'ensemble de ces considérations se dégage dès à présent une conséquence d'une remarquable généralité. C'est que les lois naturelles de l'évolution paraissent entrer dans le mouvement qui se manifeste depuis quelque temps pour les lois physiques. Elles prennent de plus en plus le caractère de lois statistiques.

Ainsi guidée par le fil conducteur de la théorie de la descendance, soumise à des mesures précises d'une parfaite rigueur mathématique et contrôlée à chaque instant par la méthode expérimentale, la Morphologie devient chaque jour davantage la science explicative par excellence du monde des êtres organisés. Les phénomènes morphologiques sont la traduction, l'expression tangible, le critérium perceptible des expériences physiologiques et celles-ci empruntent tout leur intérêt aux manifestations morphologiques qu'elles engendrent.

Avec l'élevage et l'horticulture, le morphologiste devient réellement un créateur. Il l'est encore bien mieux lorsqu'évoquant et groupant en sa pensée les conditions dans lesquelles se sont, au cours des siècles, formés successivement les êtres vivants, il aperçoit le nexus causal qui relie les formes nouvelles à celles qui les ont précédées, et prévoit dans une certaine mesure les transformations, moins étendues sans doute, que pourront subir dans l'avenir les formes actuelles disposant encore d'une certaine plasticité.

Toutefois, en prétendant que le morphologiste joue le rôle de créateur, nous n'entendons pas affir-

mer qu'il pourrait, comme l'ont parfois demandé avec une exigence ridicule les adversaires de la théorie évolutionniste, transformer *hic et nunc* une espèce animale en une autre espèce par une simple modification de la nourriture et du milieu, et par exemple tirer le Bœuf du Mouton en plaçant ce dernier pendant quelques générations dans des condition particulièrement favorables. Un pareil résultat serait la négation même de la doctrine darwinienne qui, on le sait, tient le plus grand compte des modifications accumulées par l'hérédité et fixées d'une façon irrévocable chez les organismes définitivement adaptés.

Ce que le morphologiste peut tenter, et ce qu'il tente en effet, c'est de découvrir et d'analyser les petites variations déterminées par les facteurs primaires et de démêler ainsi comment, par une lente sommation ces variations d'abord insignifiantes se sont intégrées pour donner naissance, soit par voie continue, soit par une apparente discontinuité aux caractères beaucoup plus évidents qui séparent les espèces.

Je n'ose même croire avec quelques hardis pionniers de la science moderne que la connaissance plus parfaite des autorégulations des organismes nous permettrait peut-être de modifier ces mécanismes automatiques et d'obtenir aussi une variation *rapide* des animaux et des plantes (1).

(1) As far as I am aware no one has yet found a method of bringing about a rapid variation in animals or plants. I am inclined to believe that this failure is at least partly due to the existence of mechanism of regularisation..... We again meet with two possibilities : we shall either succeed by a series of continued slight changes in one and same form in bringing about a large transformation from the original

Après une série de transformations innombrables dont il nous est possible parfois de retrouver des traces sous formes d'empreintes fossiles dans les entrailles de la terre, la plupart des êtres vivants sont arrivés à un état d'équilibre relativement stable. Ils ont épuisé les disponibilités de ce que j'ai appelé leur *potentiel plastique*, ne peuvent plus effectuer que de faibles oscillations autour d'une position moyenne et tout changement un peu considérable dans les conditions éthologiques n'est plus susceptible en général d'être compensé par un dispositif nouveau de réactions régulatrices.

Et même pour ceux qui ont encore une réserve d'élasticité plasmatique suffisante pour permettre de nouvelles adaptations, il ne faut pas oublier qu'ils ne peuvent évoluer que dans un certain nombre de directions bien définies et qu'il y a toujours lieu de tenir compte de deux faits essentiels qui règlent les transformations désormais possibles : 1° l'indestructibilité du passé, 2° l'irréversibilité de l'évolution.

C'est là, pour le dire en passant, que gît toute la difficulté de la question de la génération spontanée ou *abiogenesis*. Si par miracle nous arrivons à produire avec de la matière non vivante un être vivant aussi simple qu'on puisse l'imaginer, cet être nouveau serait certainement différent de toutes les espèces actuellement existantes, car celles-ci ont un passé que l'autre n'aurait pas et elles portent dans leur organisme si rudimentaire qu'on le suppose, la

form, or we shall obtain the result that in each form the possibility of evolution is limited and that at a certain point the constancy of a species is reached ». J. Lœb. The limitation of biological research. (*Univ. of California Publications Physiology*. vol. I, n° 5 oct. 1903.)

trace de toutes les actions auxquelles ont été soumis leurs ancêtres.

On peut même affirmer que les Monères hypothétiques dont on provoquerait la formation par abiogénèse différeraient de celles qui ont pris naissance autrefois par le même processus. Outre que les conditions de milieu dans lesquelles elles apparaîtraient seraient nécessairement différentes, les matières organiques complexes qui serviraient à leur formation auraient aussi leur histoire, et tout nous porte à penser que les propriétés des corps bruts comme celles des êtres vivants sont dans une certaine mesure fonction de leurs antécédents.

Ainsi s'explique aussi pourquoi il existe encore aujourd'hui des formes vivantes très vieilles, mais non évoluées, parce qu'elles n'ont plus de potentiel plastique disponible et qu'elles périraient plutôt que de se transformer.

Ainsi s'explique encore combien il est vain d'espérer par des conditions spéciales de milieu élever à un degré supérieur les formes relativement inférieures de l'animalité et combien il est inutile de chercher à modifier physiquement ou moralement dans un sens voulu des races considérées à tort où à raison comme relativement inférieures mais en tout cas autrement différenciées. L'évolution n'est pas reversible et on ne peut par aucun procédé faire remonter un être vivant vers le point où il est séparé de son phylum originel pour lui faire suivre ensuite une voie différente de celle qu'il avait primitivement adoptée (1).

(1) La généralité des processus pœcilogoniques montre l'instabilité de l'évolution. Or d'après Brillouin l'irréversibilité s'introduit en Mécanique rationnelle avec l'instabilité, L'irré-

Mais les limites imposées par la nature à notre science ne doivent pas nous empêcher d'en admirer la grandeur et d'en constater le prodigieux développement.

Il ne faut jamais douter du progrès. Il y a près de trente ans, au cours d'une leçon sur les premières phases du développement de l'œuf animal je disais non sans regret : « La Morphodynamique soupçonnée par Lamarck, à peine abordée par quelques rares biologistes est un territoire scientifique que la plupart des naturalistes de nos jours ne verront que comme Moïse vit la Terre promise seulement de loin, et sans pouvoir y entrer » (1).

Mes espérances ont été largement dépassées par la réalité. Sous les noms de Mécanique embryologique (Entwicklungsmechanik), de Biomécanique, de Biométrique, etc. les terres nouvelles vers lesquelles au début de ma carrière j'orientais mon voyage d'exploration scientifique ont été en partie reconnues et défrichées par de jeunes et vaillants investigateurs. Le progrès scientifique suit une progression géométrique dont la raison va sans cesse en croissant. Tel un fleuve aux eaux impétueuses, grossi par les apports de nombreux affluents dont

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versibilité qui est le caractère presque universel des phénomènes naturels réalisés en un temps fini, n'est nullement une objection contre l'explicatiou mécanique (Mécanique du XIXe siècle ou Mécanique plus générale qui nous fait entrevoir l'Electromagnétisme) du monde physico-chimique. Partout où l'on introduit actuellement pour aboutir à une théorie numérique, des viscosités ou des frottements, une analyse plus approfondie fera reconnaître et étudier des instabilités d'équilibre moléculaire. (MARCEL BRILLOUIN *Notice sur les travaux scientifiques*, 1904, p. 19-20.)

(1) A. GIARD, *Cours de Zoologie.* (*Bull. sc. Fr. et Belg.*, t. VIII, 1876, p. 258,)

il effectue la synthèse, la Morphologie déploie majestueusement son cours et les délicieuses sensations esthétiques que nous procure la contemplation des êtres organisés sont la moindre récompense de nos peines et de nos efforts persévérants.

Pour réaliser une œuvre d'art, que de collaborateurs anonymes viennent en aide au peintre ou au sculpteur ! L'artisan qui tisse la toile, le carrier qui fournit la pierre ont leur part de mérite dans le résultat final, et nous leur devons aussi une part de reconnaissance. Il en est de même dans nos sciences de la nature où chaque jour s'impose de plus en plus entre tous les travailleurs une étroite solidarité. Les diverses branches de la Biologie sont reliées entre elles comme nous l'avons vu par des liens multiples et enchevêtrés, et une branche spéciale telle que la Morphologie dépend non seulement du progrès des rameaux voisins mais aussi du développement des autres sciences, même de celles en apparence les plus éloignées.

La spécialisation qui devient forcément de plus en plus intense rend aussi plus désirables les efforts synthétiques et la coordination des résultats.

Souhaitons donc que, dans un avenir prochain, une organisation collectiviste de travail remplace l'état anarchique qui existe aujourd'hui et qui absorbe inutilemeut tant d'activités dont on pourrait faire un meilleur emploi en les hiérarchisant et les dirigeant vers un but commun.

La solidarité scientifique doit être la préface, et le modèle, de la solidarité sociale.

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British Association for the Advancement of Science.

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ADDRESS

TO THE

ANTHROPOLOGICAL SECTION

BY

A. C. HADDON, Sc.D., F R.S.

PRESIDENT OF THE SECTION.

THERE are various ways in which man can study himself, and it is clearly impossible for me to attempt to give an exposition of all the aims and methods of the anthropological sciences; I propose, therefore, to limit myself to a general view of South African ethnology, incidentally referring to a few of the problems that strike a European observer as needing further elucidation. It seems somewhat presumptuous in one who is now for the first time visiting this continent to venture to address a South African audience on local ethnology, but I share this disability with practically all students of anthropology at home, and my excuse lies in the desire that I may be able to point out to you some of the directions in which the information of anthropologists is deficient, with the hope that this may be remedied in the immediate future.

Men are naturally apt to take an exclusive interest in their immediate concerns, and even anthropologists are liable to fall into the danger of studying men's thoughts and deeds by themselves, without taking sufficient account of the outside influences that affect mankind.

In the sister science of zoology, it is possible to study animals as machines which are either at rest or in motion: when they are thus studied individually, the subjects are termed anatomy and physiology; when they are studied comparatively, they are known as comparative anatomy or morphology and comparative physiology. The study of the genesis of the machine is embryology, and palæontologists, as it were, turn over the scrap-heap. All these sciences can deal with animals irrespective of their environment, and perhaps for intensive study such a limitation is temporarily desirable, but during the period of greatest specialisation there have always been some who have followed in the footsteps of the field naturalist, and to-day we are witnessing a combination of the two lines of study.

Biology has ceased to be a mixture of necrology and physiology; it seeks to obtain a survey of all the conditions of existence, and to trace the effects of the environment on the organism, of the organism on the environment, and of organism upon organism. Much detailed work will always be necessary, and we shall never be able to do without isolated laboratory work; but the day is past when the amassing of detailed information will satisfy the demands of science. The leaders, at all events, will view the subject as a whole, and so direct individual labour that the hewers of wood and drawers of water, as it were,

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shall not mechanically amass material of which no immediate use can be made, but they will be so directed that all their energies can be exercised in solving definite problems or in filling up gaps in our information, with knowledge which is of real importance.

This tendency, which I have indicated as affecting the science of zoology, is merely one phase of an attitude of mind that is influencing many departments of thought. There are psychologists and theologians who deem it worth while to find out what other people think and believe. Arm-chair philosophers are awakening to the fact that their studies have hitherto been confined almost exclusively to the most highly specialised conditions, and that in order to comprehend these fully it is necessary to study the less and the yet less specialised conditions; for it is only possible to gain the true history of mind or belief by a combination of the observational with the comparative method. A considerable amount of information has already been acquired, but in most departments of human thought and belief vastly more information is needed, and hitherto the reliability of a great deal that has been published is not above suspicion.

The comparative or evolutionary historian also needs reliable facts concerning the social condition of varied peoples in all stages of culture. The documentary records of history are too imperfect to enable the whole story to be unravelled, so recourse must be had to a study of analogous conditions elsewhere for side-lights which will cast illuminating beams into the dark corners of ancient history. When the historian seriously turns his attention to the mass of data accumulated in books of travel, in records of expeditions, or the assorted material in the memoirs of students, he will doubtless be surprised to find how much there is that will be of service to him.

Sociologists have not neglected this field, but they need more information and more exhaustive and precise analyses of existing conditions. The available material is of such importance and interest, that the pleasure of the reader is apt to dull his critical faculty; as a matter of fact, the social conditions of extremely few peoples are accurately known, and sooner or later—generally sooner—the student finds his authorities failing him from lack of thoroughness.

I have alluded to the subjects of psychology, theology, history, and sociology, because they all overlap that area over which the anthropologist prowls. Indeed it is our work to collect, sift, and arrange the facts which may be utilised by our colleagues in these other branches of inquiry, and to this extent the ethnologist is also a psychologist, a theologian, a historian, and a sociologist.

Similarly the anthropographer provides material for the biologist on the one hand, and for the geographer on the other.

As a general rule those who have investigated any given people in the field have alluded to the general features of the country they inhabit, so that usually it is possible to gain some conception of them in their natural surroundings. Thus, to a certain extent, materials are available for tracing that interaction between life and environment and between organisms themselves, to which the term Œcology is now frequently applied, but we still need to have this interdependence more recognised in such branches of inquiry as descriptive sociology or religion.

Just as the arts and crafts of a people are influenced by their environment, so is their social life similarly affected, and their religion reflects the stage of social culture to which they have attained: for it must never be overlooked that the religious conceptions of a people cannot be thoroughly understood apart from their social, cultural, and physical conditions.

This may appear a trite remark, but I would like to emphasise the fact that very careful and detailed studies of definite or limited areas are urgently needed, rather than a general description of a number of peoples which does not exhaust any one of them—in a word, what we now need is thoroughness.

Three main groups of indigenous peoples inhabit South Africa:—The Bushmen, the Hottentots, and various Bantu tribes; in more northerly parts of the Continent there are the Negrilloes, commonly spoken of as Pygmies, the Negroes proper, and Hamitic peoples, not to speak of Arab and Semitic elements.

Kattea.

Before proceeding further I must here make allusion to an obscure race who may possibly be the true aborigines of Africa south of the Zambesi. These are the Kattea—or Vaalpens, as they are nicknamed by the Boers, on account of the dusty colour their abdomen acquires from the habit of creeping into their holes in the ground—who live in the steppe region of the North Transvaal, as far as the Limpopo. As their complexion is almost a pitch black, and their stature only about 1.220 m. (4 ft.), they are quite distinct from their tall Bantu neighbours and from the yellowish Bushmen. The 'Dogs,' or 'Vultures,' as the Zulus call them, are the 'lowest of the low,' being undoubtedly cannibals and often making a meal of their own aged and infirm, which the Bushmen never do. Their habitations are holes in the ground, rock shelters, and lately a few hovels. They have no arts or industries, nor even any weapons except those obtained in exchange for ostrich feathers, skins, or ivory. Whether they have any religious ideas it is impossible to say, all intercourse being restricted to barter carried on in a gesture language, for nobody has ever yet mastered their tongue, all that is known of their language being that it is absolutely distinct from that of both the Bushman and the Bantu. There are no tribes, merely little family groups of from thirty to fifty individuals, each of which is presided over by a headman, whose functions are acquired, not by heredity, but by personal qualities. I have compiled this account of this most interesting people from Professor A. H. Keane's book, 'The Boer States,' in the hope that a serious effort will be made to investigate what appears to be the most primitive race of all mankind. So little information is available concerning the Kattea that it is impossible to say anything about their racial affinities.

Perhaps these are the people referred to by Stow (p. 40), and possibly allied to these are the dwarfs on the Nosop River mentioned by Anderson; these were 1.125 m. (4 ft. 4 in.) or less in height, of a reddish-brown colour, with no forehead and a projecting mouth; Anderson's Masara Bushmen repudiated any suggestion of relationship with them, saying they were 'monkeys, not men.'

Bushmen.

The San, or Bushmen (Bosjesman of Colonial Annals), may, with the possible exception of the Kattea, be regarded as the most primitive of the present inhabitants of South Africa; according to most authors, there is no decisive evidence that there was an earlier aboriginal population, although M. G. Bertin informs us that Bushman tales always speak of previous inhabitants.

The main physical characteristics of the Bushmen are a yellow skin, and very short, black woolly hair, which becomes rolled up into little knots; although of quite short stature, with an average height of 1.529 m. (5 ft. 0¼ in.), or, according to Schinz, 1.570 m. (5 ft. 1¾ in.), they are above the pygmy limit of 1.450 m. (4 ft. 9 in.). The very small skull is not particularly narrow, being what is termed sub-dolichocephalic, with an index of about 75, and it is markedly low in the crown; the face is straight, with prominent cheekbones and a bulging forehead; the nose is extremely broad—indeed, the Bushmen are the most platyrrhine of all mankind; the ear has an unusual form, and is without the lobe. Their hands and feet are remarkably small.

Being nomadic hunters the Bushmen could only attain to the rudiments of material culture. The dwellings were portable, mat-covered, dome-shaped huts, but they often lived in caves; the Zulus say 'their village is where they kill game; they consume the whole of it and go away.' Clothing consisted solely of a small skin; for weapons they had small bows and poisoned arrows. Their only implement was a perforated rounded stone into which a stick was inserted; this was used for digging up roots. A very little coarse pottery was occasionally made. Although with a great dearth of personal ornaments, they had a fair amount of pictorial skill, and were fond of decorating their rock shelters with spirited coloured representations of men and animals. They frequently cut off the

terminal joint of a little finger. They never were cannibals. Cairns of stones were erected over graves. Although they are generally credited with being vindictive, passionate, and cruel, they were as a matter of fact always friendly and hospitable to strangers till dispossessed of their hunting grounds. They did not fight one another, but were an unselfish, merry, cheerful race with an intense love of freedom.

A great mass of unworked material exists for the elucidation of the religious ideas, legends, customs, and so forth, of the Bushmen, in the voluminous native texts, filling eighty-four volumes, to the collection of which the late Dr. Bleek devoted his laborious life. This wonderful collection of the folklore of one of the most interesting of peoples still remains inaccessible to students in the Grey Library in Cape Town. A more enlightened policy in the past would have enabled Dr. Bleek to publish his own material; now the task is complicated by the great difficulty of finding competent translators and of securing the services of reliable natives who know their own folklore. The time during which this labour can be adequately accomplished is fleeting rapidly, and once more the Government must be urged to complete and publish the life-work of this devoted scholar.

The Mañanja natives, who live south of Lake Shirwa, assert that formerly there lived on the upper plateau of the mountain mass of Mlanje a people they call Arungu, or 'gods,' who from their description must have been Bushmen. Relics of Bushman occupation have been found in the neighbourhood of Lakes Nyassa and Tanganyika. West of the Irangi plateau in German East Africa, between the steppes occupied by the Wanyamwezi and the Masai, live the Wasandawi, a settled hunting people who, according to Baumann, are very different from the surrounding Bantu peoples, and who are allied to the more primitive, wandering, hunting Wanege, or Watindiga, of the steppes near Usukuma. They use the bow and poisoned arrow. Their language, radically distinct from Bantu, is full of those strange click sounds which are so characteristic of Bushman speech; but Sir Harry Johnston says that he does not know if any actual relationship has been pointed out in the vocabulary, and he distinctly states that the Sandawi are not particularly like the Bushmen in their physique, but more resemble the Nandi; and Virchow declares there is no relationship between the Wasandawi and the Hottentot in skull-form. Until further evidence is collected, one can only say that there may have been a Bushman people here who have become greatly modified by intermixture with other races. Sir Harry Johnston thinks that possibly traces of these people still exist among the flat-faced, dwarfish Doko, who live to the north of Lake Stephanie, and he is inclined to think that traces of them occur also among the Andorobo and Elgunono.

If the foregoing evidence should prove to be trustworthy, it would seem that at a very early time the Bushmen occupied the hunting grounds of tropical East Africa, perhaps even to the confines of Abyssinia. They gradually passed southwards, keeping along the more open grass lands of the eastern mountainous zone, where they could still preserve their hunting method of life, until, when history dawned on the scene, they roamed over all the territory south of the Zambesi, with the exception of the eastern seaboard.

Negrilloes.

Material does not at present exist for an exhaustive discussion of the exact relationship between the Bushmen and the Negrilloes of the Equatorial forests. On the whole I am inclined to agree with Sir Harry Johnston, who says: 'I can see no physical features other than dwarfishness which are obviously peculiar to both Bushmen and Congo Pygmies. On the contrary, in the large and often protuberant eyes, the broad flat nose with its exaggerated alæ, the long upper lip and but slight degree of eversion of the inner mucous surface of the lips, the abundant hair on head and body, relative absence of wrinkles, of steatopygy, and of high protruding cheekbones, the Congo dwarf differs markedly from the Hottentot-Bushman type.' Shrubsall had previously stated: 'For the present I can only say that the data seem to me too insufficient to enable the affinities of the various pygmy races to be clearly demonstrated, or to allow of much significance being attached to any

apparent resemblance.' Deniker also draws attention to the physical characters that distinguish those two types, and he concludes that, 'nothing justifies their unification.'

Hottentots.

The skin of the Hottentots, or Khoikhoi, as they style themselves, is of a brownish-yellow, with a tinge of grey, sometimes of red; the hair is very similar to that of the Bushmen; the average stature is 1.604 m. (5 ft. 3 in.); the head is small and distinctly dolichocephalic (74), the jaws prognathic, cheekbones prominent, and chin small. Shrubsall, who has investigated the osteological evidence, says no hard-and-fast line can be drawn from craniological evidence between Hottentots and Bushmen on the one hand and Negroid races on the other, various transitional forms being found; but Bushman characteristics undoubtedly predominate in the true Hottentots.

The Hottentots were grouped in clans, each with its hereditary chief, whose authority, however, was very limited. Several clans were loosely united to form tribes. Their principal property consisted of horned cattle and sheep; the former were very skilfully trained. The dwellings were portable, mat-covered, dome-shaped huts. For weapons they had a feeble bow with poisoned arrows, but they also had assagais and knobkerries, or clubbed sticks used as missiles; coarse pottery was made. They were often described as mild and amiable.

The Hottentot migration from the eastern mountainous zone took place very much later than that of the Bushmen, and it seems to have been due mainly to the pressure from behind of the waxing Bantu peoples. These pastoral nomads took a south-westerly course across the savanna country, and if the tsetse fly had the same distribution then as now they probably, more or less, followed the right bank of the Zambesi, then struck across to the Kunene north of the desert land, and worked their way down the west coast and along the southern shore of the continent.

What is now Cape Colony was inhabited solely by Bushmen and Hottentots at the time of the arrival of the Europeans. As the latter expanded they drove the aborigines before them, but in the meantime mongrel peoples had arisen, mainly of Boer-Hottentot parentage, who also were forced to migrate. Those of the Cape Hottentots, who were not exterminated or enslaved, drifted north and found in Bushman Land an asylum from their pursuers. The north-east division of the Hottentots comprises the Koranna, or Goraqua; they were an important people, despite the fact that they had no permanent home. They migrated along the Orange River—one section went up the right bank of the Harts and the other went up the Vaal till they were deflected by the Bechuana. When the Boers in 1858 were engaged with the Basuto, the Koranna devastated the Orange Free State, but were themselves ultimately destroyed. The original home of the Griqua was in the neighbourhood of the Olifant River; in the middle of the eighteenth century the colonists settled in the land, and as a result the Griqua-Bastards retreated to the east under the leadership of the talented Adam and Cornelius Kok. They adopted the name Griqua in place of the earlier one of Bastard; one split founded Griqua Town in Griqua Land West, but the other went further east and eventually settled east of the Drakensberg, between Natal and Basuto Land, and occupied the country devastated by Chaka's wars. Here rose the chief town, Kokstadt, in Griqua Land East, where a few Griqua still live. The interesting little nation of the Bastards, descendants of unions between Europeans, mostly Boers, and Hottentot women, now mixes very little with other peoples. They were forced in 1868 to leave their home in Great Bushman Land owing to the ravages of Bushmen and Koranna, and finally, after various wanderings and viscissitudes, they settled as four communities in Great Namaqua Land, in German territory. Namaqua Land is too infertile to attract colonists, and thus it forms an asylum for expatriated Hottentots as well as for the Namaqua division of the Hottentots, the original inhabitants of the country.

True Negroes.

One of the most primitive populations of Africa is that of the true, or West African, Negroes. At present this element is mainly confined to the Sudan and the Guinea Coast.

The main physical characteristics of the true Negro are: 'black' skin, woolly hair, tall stature, averaging about 1.730 m. (5 ft. 8 in.), moderate dolichocephaly, with an average cephalic index of 74–75. Flat, broad nose, thick and often everted lips, frequent prognathism.

West African culture contains some characteristic features. The natives build gable-roofed huts; their weapons include spears with socketed heads, bows tapering at each end with bowstrings of vegetable products, swords and plaited shields, but no clubs or slings. Among the musical instruments are wooden drums and a peculiar form of guitar, in which each string has its own support. Clothing is of bark-cloth and palm-fibre, and there is a notable preponderance of vegetable ornaments. Circumcision is common and the knocking out of the upper incisors. With regard to religion, there is a great development of fetishism and incipient polytheistic systems. Colonel Ellis has proved in a masterly manner the gradual evolution of religion from west to east along the Guinea Coast, and this is associated with an analogous progress in the laws of descent and succession to property, and in the rise of government. He further suggests that differences in the physical character of each country in question have played a great part in this progressive evolution. Here also are to be found secret societies, masks and representations of human figures. The ordeal by poison is employed, chiefly for the discovery of witchcraft; anthropophagy occurs. The domestic animals are the dog, goat, pig, and hen. Cattle are absent owing to the tsetse fly. The plants originally cultivated were beans, gourds, bananas, and perhaps earth-nuts. Coiled basketry and head-rests are absent.

That branch of the true Negro stock which spake the mother-tongue of the Bantu languages some 3,000 years ago (according to Sir Harry Johnston's estimate) spread over the area of what is now Uganda and British East Africa. In the Nile valley these people probably mixed with Negrilloes, and possibly with the most northerly representatives of the Bushmen in the high lands to the east. Here also they came into contact with Hamitic peoples coming down from the north, and their amalgamation constituted a new breed of Negro—the Bantu. We have already seen what are some of the more important physical characteristics of the Negro, Negrillo, and Bushman stocks; it only remains to note in what particulars they were modified by the new blood.

Hamites.

The Hamites are to be regarded as the true indigenous element in North Africa, from Morocco to Somaliland. Two main divisions of this stock are generally recognised: (1) the Northern or Western Hamites (or Mediterranean race of some authors), of which the purest examples are perhaps to be found among the Berbers; and (2) the Eastern Hamites or Ethiopians. These two groups shade into each other, and everywhere a Negro admixture has taken place to a variable extent since very early times. The Hamites are characterised by a skin-colour that varies considerably, being white in the west and various shades of coffee-brown, red-brown, or chocolate in the east; the hair is naturally straight or curly, but usually frizzly in the east. The stature is medium or tall, averaging about 1.670 m. (5 ft. 5¾ in.) to about 1.708 m. (5 ft. 7¼ in.); the head is sub-dolichocephalic (75–78); the face is elongated and the profile not prognathous; the nose prominent, thin, straight, or aquiline, with narrow nostrils; lips thin or slightly tumid, never everted.

Bantu.

Roughly speaking, the whole of Africa south of the equator, with the exception of the dwindling Bushman and Hottentot elements, is inhabited by Bantu-speaking peoples, who are extremely heterogeneous, but who exhibit

sufficient similarities in physical and cultural characteristics to warrant their being grouped together: the true Negro may be regarded as a race; the Bantu are mixed peoples.

It will be noticed that as a rule the Bantu approach the Hamites in those physical characters in which they differ from the true Negroes, and owing to the fact that the physical characters of Semites in the main resemble those of Hamites, any Semitic mixture that may have taken place will tend in the same direction as that of the Hamitic. The diversity in the physical characters of the Bantu is due to the different proportions of mixture of all the races of Africa. What we now require is a thorough investigation of these several elements in as pure a state as possible, and then by studying the various main groups of Bantu peoples their relative amount of racial mixture can be determined.

The physical characteristics of the Bantu vary very considerably. The skin colour is said to range from yellowish-brown to dull slatey-brown, a dark chocolate colour being the prevalent hue. The character of the hair calls for no special remark, as it is so uniformly of the ordinary Negro type. The stature ranges from an average of about 1.640 m. (5 ft. 4½ in.) to about 1.715 m. (5 ft. 7½ in.). Uniformity rather than diversity of head-form would seem to be the great characteristic of the African black races, but a broad-headed element makes itself felt in the population of the forest zone and of some of the upper waters of the Nile Valley. It appears that the broadening of the head is due to mixture with the brachycephalic Negrillo stock, for, whereas the dolichocephals are mainly of tall stature, some of the brachycephals, especially the Aduma of the Ogowe, with a cephalic index of 80·8, are quite short, 1.594 m. (5 ft. 2¾ in.) The character of the nose is often very useful in discriminating between races in a mixed population, but it has not yet been sufficiently studied in Africa, where it will probably prove of considerable value, especially in the determination of amount of Hamitic or Semitic blood. The results already obtained in Uganda are most promising. Steatopygy is not notable among men; fatty deposits are well-developed among women, but nothing approaching the extent characteristic of the Hottentots and Bushmen.

It appears that the Bantu peoples may be roughly divided according to culture into two groups: a western zone, which skirts the West African region and extends through Angola and German West Africa into Cape Colony; and an eastern zone. (1) The western Bantu zone is characterised by beehive huts, the absence of circumcision, and the presence of wooden shields (plain or covered with cane-work) in its northern portion, though skin shields occur to the south; (2) In the eastern Bantu zone the huts are cylindrical, with a separate conical roof.

Certain characteristics are typical of the Bantu culture. The natives live in rounded huts with pointed roofs; their weapons comprise spears, in which the head is fastened into the shaft by a spike, bows of equal thickness along their length, with bowstrings of animal products, clubs and skin shields, but slings are usually absent; the clothing is of skin and leather, and there is a predominance of animal ornaments; knocking out of the lower incisors is general, circumcision is common, though among the Kafir tribes it seems to be dying out; ancestor-worship is the prevalent form of religion, fetishism and polytheism are undeveloped; masks and representations of human figures are rare, and there are no secret societies; anthropophagy is sporadic and usually temporary; the domestic animals include the dog, goat, and sheep, and cattle are found wherever possible; coiled basketry is made, and head-rests are a characteristic feature.

M. A. de Préville has drawn a broad line of distinction between the religion of the pastoral Bantu tribes and that of the hunters of the forest belt. The cattle-raisers of the small pastures recognise that the rain and necessary moisture depend on an invisible and supreme power whom they invoke in his location in the sky. His intermediaries are the rain-makers, he has no human form, neither are there idols in the pantheon. In Central Africa there is more than sufficient rain, but rain is of little importance to the hunter. What he requires is to find game, to be able to capture it and to avoid danger; the 'medicine-men'

are not rain-makers, but makers of talismans, amulets, philtres, and charms to attract the game and to ensure its capture. The mysterious depths of the forest, in the impenetrable thickets of which death may lurk at each step, and the isolation which results in social disorganisation, incline the hunter to superstitious terrors. Pasturage is governed by natural impersonal forces, but hunting is individual and personal. Further, associated with the mobile pastoral life of the Bantu is the patriarchal system of family life, respect and veneration for old age, and the autocracy of the chief; no wonder, then, that ancestor-worship has developed, or that it is the chief factor in the religious life of these people.

As I have previously indicated, there is evidence of the former extension to the north of the Hottentots and the Bushmen, they having gradually been pressed first southwards and then into the steppes and deserts of South Africa by the southerly drifting of the Bantu.

The mixture of Hamite with Negro, which gave rise to the primitive Bantu stock, may have originated somewhere to the east or north-east of the Victoria Nyanza. A factor of great importance in the evolution of the Bantu is to be found in the great diversity of climate and soil in Equatorial East Africa. It is a country of small plateaus separated by gorges, or low-lying lands. The small plateaus are suitable for pasturage, but their extent is limited; thus they fell to the lot of the more vigorous people, while the conquered had to content themselves with low country, and were obliged to hunt or cultivate the land. In these healthy highlands the people multiplied, and migration became necessary; the stronger and better-organised groups retained their flocks and migrated in a southerly direction, keeping to the savannas and open country, the line of least resistance being indicated by the relative social feebleness of the peoples to the south. In the small plateaus a nomadic life is impossible for the herders, there being at most a seasonal change of pasturage, this prevents the possession of large herds and necessitates a certain amount of tillage, further, it would seem that this mode of life tends to develop military organisation and a tribal system.

No materials at present exist for any attempt at a history of this stage of the Bantu expansion, but from what we know of the great folk-wanderings in South Africa during the first half of the nineteenth century, we can form some estimate of what may have happened earlier in Equatorial Africa.

Lichtenstein lived among the Bechuanas in 1805, and from that date begins our knowledge of the Bantu peoples. Dr. G. M. Theal, the learned historian of South Africa, Dr. K. Barthel and Mr. G. W. Stow, whose valuable book has just appeared, have made most careful studies of folk-wanderings in South Africa, based upon the records of the explorers of the past hundred years; we scarcely have trustworthy accounts of the movements of the various tribes for a longer period, and oral traditions of the natives, though in the main correct, require careful handling. The nature of the country is such that it affords more than ordinary facilities for migrations, and the absence of great geographical barriers prevents ethnical differentiation.

The Bantu peoples of Southern Africa may conveniently be classified in three main groups:—

(1) The eastern tribes, composed of the Zulu-Xosa.
(2) The interior tribes, consisting of the Bechuana, Basuto, Mashona, &c.
(3) The western tribes, such as the Ovampo and Ovaherero.

(1) The Zulu-Xosa are respectively the northern and southern branches of a migration down the East Coast, that, according to some authorities, took place about the fifteenth century. The Amaxosa (Kosa, or Kafirs), never overstepped the Drakensberg range, but there have been northerly, and more especially southerly movements: the Amaxosa, for example, extended, about 1800, as far as Kaaimans River, Mossel Bay, but in 1835 they were pressed back by the colonists to the Great Fish River.

The Amazulu have occupied the east coast, north of the Tugela, for a long period, and allied tribes extend as far as the Zambesi; indeed, it may be said that a complete chain of Zulu peoples stretches up to the neighbourhood of the Equator, the

more open country in which they live giving greater opportunities for expansion. The wonderful rise to power of Chaka (1783–1828), caused great movements of peoples to take place. The Amangwane (who drove the Amahlubi before them) and other groups fled southward to escape from the tyranny of this great warrior. The conquerors applied to these scattered remnants of tribes the contemptuous term 'Fingu,' or homeless fugitives, and turned them into slaves and cattle-tenders. The Matabele, to the number of some 60,000 individuals, separated from the parent stock about 1817, under the leadership of the terrible Moselekatze (Umsilikatzi), whose fame as an exterminator of men ranks second only to that of Chaka; they crossed the Drakensberg and went north-west through the Transvaal, scattering the settled Bechuana peoples. They were attacked by the Boers, who defeated them with terrible slaughter, from which only forty warriors escaped. They withdrew to the Zambesi, but were driven south by the tsetse fly. They encountered the Makalaka and destroyed their villages, drove out the Mashona to the north-east, and settled in Mashonaland.

(2) The great central region of the South African plateau, roughly known as Bechuanaland, was very early occupied by Bantu peoples coming from the north, who displaced or reduced to servitude the indigenous Bushmen. As Professor Keane points out, the Bechuana must have crossed the Zambesi from the north at a very early date, because of all the South Bantu groups they alone have preserved the totemic system. Among the first to arrive, according to him, appear to have been the industrious Mashona and Makalaka. For three hundred years, according to native tradition, the Makalaka owned the land between the Limpopo and the Zambesi, and then came the Barotse, who are allied to the Congo Bantu and conquered them. A section of the latter founded a powerful so-called Barotse (Marotse) Empire on the Middle Zambesi above the Victoria Falls. At the beginning of the nineteenth century a Bahurutse dynasty ruled over the Bechuana; as these people expanded they broke off into clans, and extended between the Orange River and the Zambesi, and from the Kathlamba, or Drakensberg chain, to the Kalahari Desert.

The densely-populated country west of the Drakensberg now known as Basutoland was subjected to great devastation as a result of Chaka's tyranny. In 1822 a tribe fleeing from the Zulus set up the first of these disturbances, and the attacked became the attackers in their turn. One horde, the Mantati, achieved great notoriety, and are credited with having wiped out twenty-eight tribes; they were eventually defeated by the Bangwaketsi and scattered by the Griqua. The Makololo, a small group of the Mantati (who lived on the upper waters of the Orange River), led by Sebituane, in 1823 aimed at reaching the district of the Chobe and Zambesi, where he had heard that it was always spring. After conquering the Bakwena, Bahurutse and other kindred tribes and increasing their forces from the conquered peoples, they crossed the Zambesi and the uplands stretching to the Kafukwe, and settled in those fertile pasture lands about 1835. Disturbed by the Matabele, Sebituane passed through the Barotse Valley, followed by the Matabele and the Batoka, a tribe of the Barotse. He put the former to flight and subjugated the latter. Thus Sebituane led his people a journey of over 2,000 miles to reach their Promised Land. Under Sekeletu, Sebituane's successor, the State began to fall to pieces, and after his death the Barotse revolted, and practically exterminated the Makololo. The rehabilitated Barotse empire comprises an area of some 250,000 square miles between the Chobe and Kafukwe affluents of the Zambesi. Professor Keane draws attention to the instructive fact that though the Makololo have perished from among the number of South African tribes, their short rule (1835–1870) was long enough to impose their language upon the Barotse, and to this day, about the Middle Zambesi, where the Makololo have disappeared, their speech remains the common medium of intercourse throughout the Barotse empire. The consolidation of the Basuto under the astute Mosheah is an instructive episode in the history of the South African races. The Bamangwato are the most important branch of the independent Bechuana peoples, who have made considerable progress under the wise guidance of the enlightened Khama; they are an industrious people, and have exceptional skill in working iron.

According to Mr. G. W. Stow there were three main migrations of the interior, or middle, Bantu, or Bachoana as he terms them: (i) The pioneer tribes of the southward migration into the ancient Bushman hunting grounds were the Leghoya, Bakalahari, and those who intermarried with the Bushmen to form the Balala and Bachoana Bushmen; (ii) the tribes of the second period of the Bachoana migration were the Batlapin and Barolong; (iii) the great Bakuena or Bakone tribes were the most civilised of the Bantu peoples: they consisted of the Bahurutse, Batlaru, Bamangwato, Batauana, Bangwaketse, and the Bakuena, who were the wealthiest and most advanced of all until they were reduced by the Mantati and destroyed by the Matabele.

(3) Turning for a moment to German South-west Africa we find the Bastards to the south, and north of them the Haukoin or Mountain Damara, who are now practically a pariah people, subject to the Hottentots, Bastards, Ovaherero, and the white man. It is possible that these are of Negro rather than of Bantu origin; in mode of life, save for their talent for agriculture, they are Bushmen; in their speech they are Hottentots, but their colour is darker than that of their neighbours. Somewhere from Eastern South Africa, about a hundred years ago, came the Ovaherero, or the Merry People, who, like the rest of the Bantu, are warlike cattle-breeders, with wandering proclivities, but they are not agriculturists. When they arrived in the Kaoko district they drove the Haukoin to the south, together with the Toppnaers (Aunin) and Bushmen. To the north of the Ovaherero are the agricultural Ovampo.

Speaking generally, the direction of ethnic migration in South Africa has been southerly in the south-east: the sea blocked an eastern expansion and the Drakensberg a western; only the Matabele went westward of this range to the north. In the central district the Bahurutse or Bechuana parent stock dispersed in various directions; most of the movements were towards the north, but the Mantati and Basuto went south-east. In the west the Cape Hottentots always retreated from the colonists towards the north, the Bastards and other tribes followed the same direction, the causes, as Barthel points out, being obvious; to the east is the Kalahari, on the west is the sea, from the south came the pressure of the Boers. Finally, right across South Africa we have, from west to east, the Koranna, Griqua, and Boer wanderings in the south; and in the north, from east to west, the wanderings of the Hottentots, Ovaherero, and of the Boer emigrants from the Transvaal.

South Africa has thus been a whirlpool of moving humanity. In this brief summary I have been able to indicate only the main streams of movement: there have been innumerable cross-currents which add complexity to this bewildering history, and much patient work is necessary before all these complications can be unravelled and their meaning explained.

When one takes a bird's-eye view of the ethnology of South Africa, certain main sociological facts loom out amongst all the wealth of varied detail.

The earliest inhabitants of whom we have any definite information were the dwarf Bushmen, who undoubtedly represent a primitive variety of mankind. In a land abounding with game they devoted themselves entirely to the chase, supplementing their diet with fruit and roots. This mode of life necessitates nomadic habits, the absence of property entails the impossibility of gaining wealth, and thereby relieving part of the population from the daily need of procuring food; this absence of leisure precludes the elaboration of the arts of life. A common effect of the nomadic hunting life is the breaking-up of the community into small groups; the boys can soon catch their own game, hence individualism triumphs and parental authority is apt to be limited. Social control is likely to be feeble unless the religious sentiment is developed, and certainly social organisation will be very weak. In an open country abounding with game the case is somewhat different, and there is reason to believe that in early days the Bushmen were divided into a number of large tribes, occupying tolerably well-defined tracts of country, each being under the jurisdiction of a paramount chief. The tribes

were subdivided into groups under captains. They showed great attachment and loyalty to their chiefs, and exhibited a passionate love for their country. For hundreds of years these poor people have been harried and their hunting grounds taken away from them, and hence we must not judge the race by the miserable anarchic remnant that still persists in waste places. Nomad hunters do not progress far in civilisation by their own efforts, nor are they readily amenable to enforced processes of civilisation. Invariably they are pushed on one side or exterminated by peoples higher in the social scale.

When the written history of South Africa begins we find the Bushmen already being encroached upon by the Hottentots, who themselves sprang from a very early cross of Bantu with Bushmen. Culturally, as well as physically, they may be regarded as a blend of these two stocks. They combined the cattle-rearing habits of the Bantu with the aversion from tillage of the soil characteristic of the hunter; they became nomadic herders, who were stronger than the Bushmen, but who themselves could not withstand the Bantu when they came in contact with them, and they too were driven to less favourable lands and became enslaved by the invaders. All gradations of mixture took place till lusty uncontaminated Bantu folk forced their way into the most desirable districts. Still less could the Hottentots prevail against the colonists; their improvidence was increased by alcohol, and their indifference to the possession of land, due to their inherent love of wandering, completed their ruin.

The Bantu were cattle-rearers who practised agriculture. The former industry probably was transmitted from their Hamitic forefathers, who were herdsmen on the grassy uplands of north-eastern Africa, while the latter aptitude was probably due in part to their negro ancestry. This duality of occupation led to variability in mode of life. In some places the land invited the population towards husbandry, in others the physical conditions were more suited to a pastoral life, and thus we find the settled Baronga on the one hand and the wandering Ovaherero on the other. The Bantu peoples easily adopt changes of custom; under the leadership of a warlike chief they become warlike and cruel, a common characteristic of pastoral peoples, while it is recorded that many of the Matabele, taken prisoners by the Barotse, settled down peacefully to agriculture. The history of the prolific Bantu peoples on the whole indicates that they were as loosely attached to the soil as were the Ancient Germans, and like the latter, at the slightest provocation, they would abandon their country and seek another home. This readiness to migrate is the direct effect of a pastoral life, and along with this legacy of unrest their Hamitic ancestors transmitted a social organisation which lent itself to discipline. These were the materials, so to speak, ready to hand when organisers should appear. Nor have such been lacking, for such names as Dingiswayo, Chaka, Dingan, Moselekatze, Lobengula, Moshesh, Sebituane, Cetewayo, and others are writ large in the annals of South Africa; and the statesman Khama is an example of what civilisation can do to direct this executive ability into proper channels.

Archæology.

The archæology of South Africa is now attracting considerable local interest, and we may confidently expect that new discoveries will soon enable us to gain some insight into the dense obscurity of the past. It cannot be too strongly insisted upon that the methods of the archæologist should be primarily those of the geologist. Accurate mapping of deposits or localisation of finds is absolutely necessary. The workmanship of an implement is of little evidential value: the material of which it is made may be refractory, the skill of the maker may be imperfect, or he may be satisfied with producing an implement just sufficient for his immediate need; and there is always a chance that any implement may be simply a reject. The early generalisation of implements in England into two groups, Palæolithic and Neolithic, expressed a fact of prime importance, but now the classification has extended. It is obvious that the shapely palæoliths of the older gravels could not have been the first attempts at implement making by our forefathers, and the presumed hiatus between the two epochs has been bridged over

by evidence from sites on the European mainland. Our knowledge is increasing apace and an orderly sequence is emerging, but there are many interesting variations, and even apparent setbacks, in the evolution of industrial or artistic skill. In a word, sequence and technique must not be confounded, and our first business should be to establish the former on a firm basis; but, as I have just remarked, this can be accomplished only by adhering rigidly to the stratigraphical methods of the geologist. It would probably be to the interest of South African archæology if the terms 'Eolithic,' 'Palæolithic,' and 'Neolithic' were dropped, at all events for the present, and it might prove advantageous if provisional terms were employed, which could later on be either ratified or abandoned, as the consensus of local archæological opinion should decide.

In certain lands of the Old World, north of the Equator, there was a progressive evolution from the Stone Ages, through a copper and a bronze age, to that of iron; but the stone-workers of South Africa appear to have been introduced to iron-smelting without having passed through the earlier metal phases, since the occurrence of copper implements is too limited to warrant the belief that it represents a definite phase of culture. The similarity of the processes employed in working iron by the different tribes of Africa, south of the Equator, indicates that the culture was introduced from without, a conclusion which is supported by the universal use of the double bellows—a similar instrument is in use in India and in the East Indian Archipelago. Some ethnologists hold that Africa owes to India its iron industry and other elements of culture, as well as the introduction of the ox, pig, and fowl. At all events, we shall probably not be far wrong if we assign a fair degree of antiquity to the knowledge of iron in tropical and southern Africa.

The characteristic metal of South Africa is gold, and its abundance has had a profound effect on the country, although, strange to say, it was not employed by any of the native races on their own initiative. We cannot tell when it was first discovered or by whom, but the hundreds of ruins scattered over a large extent of country, and the very extensive ancient workings, testify to the importance and the long continuance of this industry; for there can be no doubt that the builders of these wonderful remains came to this country mainly for the sake of its goldfields, though there must also have been an important trade in ivory, and incidentally in other local produce. Positive demonstration is as yet lacking concerning the nationality of the first gold-workers. This much is certain: they must have come to South Africa originally for some other product, since the aborigines did not work the metal, and it is most probable their quest was for ivory, and it was these hunters and traders who discovered the surface gold. Further, the discoverers must have come from a country where quarrying and metal-smelting were practised, and this implies the organisation of labour, for in early times, as history abundantly proves, mining was always undertaken by means of forced labour. The gold-workers, who probably came from Southern Arabia, belonged to a much higher social order than any of the peoples with whom they came in contact, and with their discipline in war and their industrial training they were able to subdue the Bantu inhabitants over immense tracts between the Zambesi and the Limpopo, to reduce them to slavery, to organise the working of the gold mines, and to establish a chain of forts and a system of communication with the coast. This occupation of the country by foreigners was purely for purposes of exploitation, and when, for reasons at present unknown to us, their hold weakened on the land, the whole enterprise fell to pieces and the foreigners departed; they left indelible marks of their former presence on the face of the country, but in native industries and customs there is virtually no trace remaining of the rule of the more civilised Semitic overlord. The natives seem, as it were, to have awakened from a nightmare and straightway to have forgotten the hideous dream. Possibly this history may have been repeated more than once.

It is greatly to be deplored that in the past irresponsible prospectors have been permitted to rifle the ancient ruins for gold, with the result that not only have very numerous specimens of archæological interest been cast into the melting-pot, but at the same time collateral evidence has been destroyed, and thus valuable data lost to science. Even now the situation is not without its dangers, for the

recently awakened interest in the ruins, and appreciation of their historical value, may lead to unconsidered zeal in excavation. After all, there is no especial hurry; what is perishable has long ago decayed, and so long as the ruins are sealed up by the rubbish that preserves them, no great harm can accrue, but a few hours of careless excavation may destroy more archæological evidence than centuries of neglect. Therefore it would be advisable for those in authority to consider carefully whether it is wise to lay bare new sites, unless proper examination and preservation can be ensured. The number of the ruins in Rhodesia is so great, and the area within which they occur so enormous, that it would be a very large undertaking for the Government systematically to investigate and permanently to conserve them all. Perhaps it would be possible to entrust some of this work to properly constituted local authorities, assisting them by grants and special facilities, but care would have to be taken to ensure the thorough carrying out of the work. Records of work done should be published, and the specimens preserved in authorised museums only. It is desirable also that every ruin should be scheduled under an Ancient Monuments Protection Act, and that an Inspector or Curator of Ancient Monuments should be appointed, who would be responsible for the excavation and preservation of all the monuments. To a less extent these remarks apply also to other parts of South Africa. All relics of the past, such, for example, as the pictographs in the rock-shelters of the Bushmen, should be jealously preserved and guarded from intentional or unwitting injury.

I trust my South African colleagues will forgive me if I have appeared too much in the character of a mentor. I have endeavoured to present a general view of the anthropological situation in South Africa, without burdening my remarks with details, and at the same time I have made bold to publish some of the conclusions which this survey has suggested; but there are other points on which I feel constrained to touch.

Recently Sir Richard Temple delivered an Address on 'The Practical Value of Anthropology,' in the course of which he said: 'We often talk in Greater Britain of a "good" magistrate or a "sympathetic" judge, meaning thereby that these officials determine the matters before them with insight; that is, with a working anthropological knowledge of those with whom they have to deal. . . . It is, indeed, everything to him to acquire the habit of useful anthropological study before he commences, and to be able to avail himself practically and intelligently of the facts gleaned, and the inferences drawn therefrom, by those who have gone before him. . . . Take the universally delicate questions of revenue and taxation, and consider how very much the successful administration of either depends on a minute acquaintance with the means, habits, customs, manners, institutions, traditions, prejudices, and character of the population. In the making of laws too close a knowledge of the persons to be subjected to them cannot be possessed, and however wise the laws so made may be, their object can be only too easily frustrated if the rules they authorise are not themselves framed with an equally great knowledge, and they in their turn can be made to be of no avail unless an intimate acquaintance with the population is brought to bear on their administration. For the administrator an extensive knowledge of those in his charge is an attainment, not only essential to his own success, but beneficial in the highest degree to the country he dwells in, provided it is used with discernment. And discernment is best acquired by the "anthropological habit." . . . The habit of intelligently examining the peoples among whom his business is cast cannot be overrated by the merchant wishing continuously to widen it to profit; but the man who has been obliged to acquire this kind of knowledge without any previous training in observation is heavily handicapped in comparison with him who has acquired the habit of right observation, and, what is of much more importance, has been put in the way of rightly interpreting his observations in his youth.'

In referring to civil-servants, missionaries, merchants, or soldiers, Sir Richard Temple went on to say: 'Sympathy is one of the chief factors in successful dealings of any kind with human beings, and sympathy can only come with knowledge. And not only does sympathy come of knowledge, but it is knowledge that begets

sympathy. In a long experience of alien races, and of those who have had to govern and deal with them, all whom I have known to dislike the aliens about them, or to be unsympathetic, have been those that have been ignorant of them; and I have never yet come across a man who really knew an alien race that had not, unless actuated by race-jealousy, a strong bond of sympathy with them. Familiarity breeds contempt, but it is knowledge that breeds respect, and it is all the same whether the race be black, white, yellow, or red, or whether it be cultured or ignorant, civilised or semi-civilised, or downright savage.'

I have quoted at length from Sir Richard Temple, as the words of an administrator of his success and experience must carry far greater weight than anything I could say. I can, however, add my personal testimony to the truth of these remarks, as I have seen Britons administering native races on these lines in British New Guinea and in Sarawak, and I doubt not that I shall now have the opportunity of a similar experience in South Africa.

In this connection I ought to refer to what has been already done in South Africa by the Government. In the year 1880 the Government of Cape Colony, confronted by the problem of dealing with the natives, appointed a Commission to enquire into the native laws and customs which obtained in the territories annexed to the Colony, especially those relating to marriage and land-tenure, and to suggest legislation, as well as to report on the advisability of introducing some system of local self-government in the native territories annexed to the Colony. The example was shortly afterwards followed by the Government of Natal, which had native problems of its own. These two Commissions collected and published a considerable amount of evidence, valuable not only for the immediate purpose in view, but also for the purposes of science. Before the late war came to a close the Anthropological Institute of Great Britain and Ireland and the Folklore Society addressed to Mr. Chamberlain, then Colonial Secretary, a memorial praying that on the conclusion of peace a similar Commission should be issued to enquire into the customs and institutions of the native tribes in the Transvaal and the Orange River Colony, and, with a view to the accomplishment of more directly scientific ends, praying that at least one anthropologist of eminence unconnected with South Africa should be included in the Commission. The prayer of the Memorialists was bluntly refused. When, however, in the course of reorganisation of the administration, a conference was held at Bloemfontein in 1902 of the Ministers of the various colonies, protectorates, and territories, to discuss native affairs, they found themselves, in the words of Sir Godfrey Lagden, 'much confused because the laws and the conditions of all the colonies were different.' This was exactly what the Memorialists had told Mr. Chamberlain. So the conference determined on the appointment of a Commission of Inquiry, which was issued in due course by Lord Milner in September, 1903, and reported on January 30 last. The evidence taken by this Commission, as well as those taken by the previous Commissions, is of a very valuable character. But, like those Commissions, its object was exclusively administrative. Consequently the evidence is only incidentally of ethnological interest, and it by no means covers the whole ground. The social life and marriage laws are to a great extent laid before the reader, but there is no attempt to distinguish accurately between one tribe and another; the native institutions are discussed only so far as they have a practical bearing on administrative questions. There is no attempt to penetrate to the underlying ideas and beliefs, and the vast domain of religion lies for the most part outside the ken of the Commissioners. Admirable, therefore, as is the work done by these Commissions, it is but a small part of what must be undertaken if an accurate account of the natives of South Africa is to be obtained and preserved for scientific use, and as an historical record. What is wanted is that the Government should undertake this enterprise in the same way as that in which the Governments of the United States, Germany, the Netherlands, and of other countries investigate their native races, or, failing this obvious duty of a Government, adequate assistance should be given to societies or individuals who may be prepared to take the matter in hand.

Unfortunately it is not unnecessary to insist on the need there is for us to

consider seriously what at any particular time is most worth investigating, and not to let ourselves drift into any casual piece of work. Let us apply that simple test to South Africa, and ask ourselves, What most needs doing in anthropological research in South Africa?

So long as actual wanton destruction is not taking place, local archæological investigation can wait. I do not mean to suggest that those who have the opportunity should not devote themselves to this important subject; many can do good work in archæology who have neither opportunity nor inclination for other branches of anthropology, and the British South Africa Company has shown and probably will continue to show a real interest in this work. But our first and immediate duty is to save for science the data that are vanishing; this should be the watchword of the present day.

Observations in South African anthropography are lamentably deficient. Although scattered up and down in books of travel and in missionary records, there are descriptions of individuals, and in some cases a few salient features of a tribe are noted; yet we have few precise descriptions of communities that are of value for comparative purposes. Anthropometrical data are everywhere wanting; very few natives have been measured, and the measurements that have been made are insufficient both as regards those actually taken and the number of individuals measured. The interesting subject of comparative physiology is unworked. We have no observations in experimental psychology, and very few reliable data in observational psychology. Here, then, is a large field of inquiry.

I am not competent to speak concerning linguistics, but from what I have read I gather that a very great deal yet remains to be done, at all events in phonetics, grammar, and comparative philology.

In general ethnology a considerable amount of scattered work has been done, but no one tribe has been investigated with scientific thoroughness; the best piece of work hitherto accomplished in this direction is the admirable memoir on the Baronga by the missionary H. A. Junod, which leaves little to be desired. It would be well worth while for students to make exhaustive studies of limited groups of people, tracing all the ramifications of their genealogies in the comprehensive method adopted by Dr. Rivers for the Torres Straits Islanders and for the Todas; this method is indispensable if it is desired to obtain a true conception of the social structure of a people, their social and religious duties, the kinship relationships, and other information of statistical and sociological value. Other fruitful lines of inquiry are the significance of the form and ornamentation of objects and the symbolism (if there is any) of the decorative art, a subject which, as far as I am aware, is absolutely untouched. Even the toys and games are worth investigation. Hardest but most important of all, there is that intricate complexus of action and belief which is comprised under the term 'religion.' This needs the most delicate and sympathetic treatment, although too often it has been ruthlessly examined by those who were more prone to seek the ape and the tiger and vain imaginings in the so-called 'superstitious' practices of these poor folk. They are laggards along the road which our more favoured ancestors have trod, but they all have their faces set in the same direction as our own, towards that goal to which we ourselves are striving. To induce natives to unbosom themselves of all that they hold secret and sacred and to confess their ideals and inspirations requires more than an ordinary endowment of patience, tact, and brotherly kindness; without these qualities very little can be gathered, and the finer side of native thought and feeling will for ever remain a sealed book to the European. In referring to this subject it should not be overlooked that the best account we have of the religion of the Zulu-Xosa peoples is due to the labours of Bishop Callaway. The number of native texts, including folk-tales, published by him are especially valuable, as they throw light from all sides upon the native mind, and it is greatly to be regretted that he lacked the pecuniary and other encouragement that was necessary for the completion of his labours. The most urgent of all the foregoing lines of inquiry are the most elusive; these are the ideas, beliefs, and institutions of the people, which are far less stable than are their physical characteristics.

These are some of the lines of research that await the investigator. The field is large, but the opportunities are fleeting. The Kattea, Bushmen, and Hottentots are doomed, and new social conditions are modifying the Bantu peoples. Here again we must apply the test question, Which of these peoples most needs investigation? The answer again is obvious. Those that will disappear first. All over South Africa this work is pressing. For some tribes it is too late. It would be a memorable result of the meeting of the British Association in South Africa if it should lead to an exhaustive study of those most interesting people, the Kattea, the Bushmen, and the Hottentots. They represent very primitive varieties of mankind, but their numbers are rapidly diminishing, and, as races, they have no chance of perpetuity. What judgment will posterity pass upon us if, while we have the opportunity, we do not do our best to save the memory of these primitive folk from oblivion?

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The foregoing list of books is manifestly very incomplete. A considerable amount of information concerning the natives will be found in numerous books by missionaries, travellers, and sportsmen.

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Museums Association.

THE NORWICH CONFERENCE, 1904.

ADDRESS BY THE PRESIDENT, SIDNEY FREDERIC HARMER, Sc.D., F.R.S., Fellow of King's College, Cambridge, and Superintendent of the University Museum of Zoology.

MY first and most pleasant duty is to offer you a hearty welcome to Norwich, and to the interesting Museum in which we are to hold our meetings. In welcoming you I do not speak merely as your President. My earliest recollections are of Norwich; and I can claim to be a citizen of this ancient city, and a member of one of its Scientific Societies. Although I can thus hardly regard myself as a visitor, I cordially associate myself with you in offering the best thanks of the Association to our Norwich friends whose kindness has given us the opportunity of meeting in this place.

I hope that you will be ready to admit that the Association has been well advised in holding the present Conference in Norwich. It is one of the necessary consequences of the existence of Museums that each such institution, like the organisms illustrated by its collections, has been affected by its environment in various ways, which have resulted in its differentiation from all others. I venture to think that there are few Museums in this country which have acquired more distinctive characters than the Norwich Museum. The history of the Museum has been published on more than one occasion, and I do not propose to go over the ground at any length, particularly as we may hope to be more fully informed on this subject during the course of our meetings. But I cannot refrain from expressing the gratitude which I am sure must be felt by all who have the interests of Museums at heart, to those who in a critical period were mainly responsible for securing and developing this building for the purposes for which it is at present used. This was in the first instance the result of the public-spirited action of the Corporation of the

City of Norwich, who in 1887 purchased from the Government the buildings which had been used as the County prison. The scheme for utilising the prison buildings as a Museum was conceived by Mr. E. Boardman, the City Architect, to whom is due the admirable arrangement which now exists. The old floors and partition-walls were removed from the separate blocks, and sky-lights were inserted. The corridor which connected the buildings, radiating from a central point, was modified; and the result is a series of well-proportioned and well-lighted rooms, convenient for the display of specimens, with moreover the unique addition of the Norman Keep, which has been incorporated into the series. The scheme for the removal of the collections from their former inadequate home was rendered possible by the munificence of many friends who gave it their hearty support, and most of all by Mr. John Gurney, who was Mayor of Norwich at the time when the transference was proposed. No small part of the success of the scheme has been due to the labours of Mr. James Reeve, on whom the burden of the removal chiefly fell, and who carried out the re-arrangement of the specimens in their new quarters.

While the Museum buildings are thus interesting in not being on entirely conventional and traditional lines, and in including one feature of very special architectural and archæological interest, there is much in the collections themselves which well merits your attention. I leave it to our friends on the spot to explain the characteristics of the contents of the buildings.

There is yet another reason for feeling that this City deserved a visit from the Museums Association. Norwich has on many occasions numbered among its residents men who have been distinguished in literature, science or art. The Norwich School of artists achieved a wide and lasting reputation. The City is proud to have been the home of Sir Thomas Browne, whose acute and interesting observations on the Natural History of Norfolk have recently been rendered more accessible by my friend Mr. Thomas Southwell.[1]

[1] "Notes and Letters on the Natural History of Norfolk." From the MSS. of Sir Thomas Browne. With notes by T. Southwell. 1902.

Sir James Edward Smith, Founder of the Linnean Society, and the author of numerous works on Botany, was born at Norwich in 1759. Soon after his marriage, in 1796, he took up his permanent residence in this city, where he died in 1828. In 1784, while still a young man, he had become the possessor, by purchase, of the library and Natural History collections made by Linnaeus. These collections were at first placed in a house which he had taken at Chelsea; but after his death they were bought by private subscription and presented to the Linnean Society, of which he had been President from its foundation in 1788.

The study of Botany has flourished with great vigour at Norwich and in its neighbourhood. In an interesting notice of several Norwich Botanists,[1] Sir James Smith states that in this city "the writings and merits of Linnaeus were perhaps more early, or at least more philosophically, studied and appreciated, than in any other part of Britain." Dawson Turner, "eminent as a scholar, botanist, antiquarian and bibliophile," belonged to the neighbouring town of Yarmouth. He was the father-in-law of Sir William Hooker,[2] the first Director of the Royal Botanic Gardens at Kew, who was born at Norwich in 1785, and whose son, Sir Joseph Hooker, was a native of Halesworth, a Suffolk village which is but a short distance from Norwich. John Lindley, born in 1799 at Catton, one of the suburbs of Norwich, was another of the eminent Botanists whom this city is able to claim.

The Geographical position of Norfolk has been partly responsible for the interest which has been taken in Ornithology in this county. In this connexion there is a special reason for mentioning the name of Mr. J. H. Gurney, whose unrivalled collection of Raptorial Birds is the most striking feature of the Norwich Museum. Another of the names which deserves mention in this place is that of the late Mr. John Gunn, a native of Irstead, in Norfolk, and for forty

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[1] "Biographical Memoirs of several Norwich Botanists," *Trans. Linn. Soc.*, vii., 1804, p. 295.

[2] See "A Sketch of the Life and Labours of Sir William Jackson Hooker," by Sir J. D. Hooker. *Ann. of Botany*, xvi., No. lxiv., Dec., 1902.

years Rector of that parish.[1] His collection of Mammalian remains from the Forest Bed of the East Anglian Coast forms another of the prominent features of this Museum.

Although I have mentioned but a few of those who have kept alive in Norwich the spirit of scientific research, I have perhaps said enough to show that the progress of art and learning has been materially aided by work which has been done in this city.

From the place of meeting one turns naturally to the Association itself, and to the subject with which it deals. Our Association is still very youthful, but its name constitutes a link with the past, and takes us back to the most flourishing periods of Greek culture. Here I venture to approach a subject on which I am not qualified to speak, but on which information has most kindly been given to me by Dr. Henry Jackson, of Trinity College, Cambridge. The history of the word "Museum" is closely bound up with the history of Philosophy; and although in its original connexion the word had a sense widely different from that in which it is at present used, the Museum of classical times had this in common with the Museum of to-day, that it was an intellectual centre in which the object men strove for was one of the numerous phases of the increase of human knowledge.

The story may commence with the visits paid by Plato to Sicily. An account of these visits is given by Plutarch,[2] and more particularly by Diogenes Laertius,[3] who relates the incidents of the first visit as follows:[4] —

"Plato made three visits to Sicily. The first time he went there that he might see the island and its volcanoes: and

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[1] "Memorials of John Gunn," edited by H. B. Woodward and E. T. Newton, 1891.

[2] "Plutarch's Lives," Ed. Clough, Vol. V., 1874, under "Dion," pp. 247 *f*. See also Grote, "A History of Greece," Vol. XI., 1853, p. 52, and "Plato and the other Companions of Sokrates," Vol. I., 1865, p. 120.

[3] Diogenes Laertius, "De clarorum philosophorum vitis," Ed. Cobet. Paris, 1862, p. 73. See also T. Stanley, "The History of Philosophy," 3rd ed., London, 1701, p. 166.

[4] The translation, which is nearly literal, has been kindly given to me by Dr. Jackson. The accuracy of the story must not be accepted without reserve.

Dionysius the elder, the reigning tyrant of Syracuse, required his attendance at Court. One day in conversation Plato maintained that what a tyrant found to be expedient was not necessarily good unless the tyrant was morally a superior person; and this nettled Dionysius, who lost his temper, and told Plato that he talked like an old fool. 'And you talk like an old tyrant,' said Plato. The tyrant was so furious that he made up his mind to put him to death; but Dion and Aristomenes talked him out of this resolution, and he contented himself with delivering Plato into the hands of Pollis, a Spartan diplomatist who chanced to be at Syracuse, that he might sell him into slavery. Pollis took him to Aegina, and offered him for sale there. . . . The result was that he was ransomed for 20 minas [£80], or according to others 30 minas [£120], and sent home to his friends, by Anniceris of Cyrene, who chanced to be there at the time. The friends promptly sent Anniceris the money; but he declined it, remarking that they were not the only people in the world worthy to take an interest in Plato. Some say that Dion sent the money, and that Anniceris would not have it, and used it to buy for Plato the garden of the Academy."

It is this reference to the Academy which justifies the introduction of the story of Plato.

On Plato's return from Sicily, about 386 B.C.[1] he commenced his formal public teaching in a garden near the precinct sacred to the Hero Hecademus or Academus. Here, under the name of the Academy, was founded the earliest of the Athenian schools of philosophy, which outlived the other similar institutions until it was finally closed by Justinian in 529 A.D. These philosophical schools were institutions which were in many respects similar to our own modern colleges.[2] Here I cannot do better than quote Dr. Jackson's actual words:—

"In the eye of the law they were religious Societies (*θίασοι*) for the worship of the Muses, who were patrons of the arts in general, and in particular of the art of

[1] Cf. Grote's "Plato," I., p. 121.

[2] For some account of this subject, cf. W. W. Capes, "University Life in Ancient Athens," London, 1877.

music, and accordingly they had a chapel dedicated to those divinities (Μουσεῖον). They had regular feast days in honour of the Muses, on which the members of the Society dined together. Besides the chapel, they had gardens, walks, hall, lecture rooms, cloister, library, and chambers for teachers and for students. In the Μουσεῖον or Chapel (ἱερόν) there were statues in honour of divinities, e.g., the Graces, and of the great men of the school. They had endowments, and after a time subventions from the state."

At a somewhat later period the word Μουσεῖον appears at Alexandria,[1] and in this connexion many of us made our first acquaintance with it in the pages of Kingsley's "Hypatia." The great institution known by that name is usually believed to have been founded by Ptolemy II., whose reign commenced in 285 B.C. Some account of it is given by Strabo, who visited Alexandria in 24 B.C., and it appears that its character was distinctly Collegiate. It possessed a Hall, in which the members of the Museum had their meals, together with cloisters and other buildings. The original religious meaning of Μουσεῖον is still retained, as is indicated by the fact that the president was styled "the priest." The members of the Museum probably received annual stipends.

It is not clear whether the Great Library at Alexandria, founded and developed by Ptolemy I and II, formed part of the buildings of the Museum: but in any case it seems to have been near them, and it is expressly stated by Strabo that the Museum and Library at Alexandria were founded in imitation of the school of Aristotle and Theophrastus at Athens.[2] It is of interest to us, in glancing back at our prototype, to be informed that it was the practice "at the Alexandrine library, to keep a faithful record of the person and quarter from whence each book had been acquired."[3] The accurate registration of specimens, as practised in modern Museums, is here more than foreshadowed.

It is thus "easy to understand how the word μουσεῖον, losing religious significance, came to imply solely places of

[1] See J. E. Sandys "A History of Classical Scholarship," 1903, p. 105.

[2] Grote's "Plato," I, p. 146.

[3] *Ibid.*, p. 155 n.

learning and art,"[1] though Dr. Jackson informs me that he sees no indication that the word was used in this sense, as a common noun, before mediæval times.

We have no reason to be dissatisfied with the name which we have inherited. Our objects are in a broad sense the same as those of men who lived more than two thousand years ago, namely, "the encouragement of education, learning and research," and even though we do not look at these matters with the eyes of the past, the fact that our point of view has somewhat shifted in no way affects the sympathy we feel for all whose aims have at any time been the preservation and the increase of human knowledge. These two objects are still our own. We are indeed largely concerned with the preservation of knowledge, which, without our efforts, might be lost to the world; but we are conscious of the fact that many lines of research can only be pursued with the aid of the specimens which it is our function to accumulate and to preserve.

But our Association is a practical body; and however interesting it may be to enquire into the history of our subject, it is of more immediate concern to us to consider the lines on which we can best work for the advancement of learning. This question has frequently been discussed by the Association, but it is one which offers so much scope for differences of opinion that it will be long before the last word is said about it.

It appears to me that some of the advice which has been given by experts with regard to the formation of an ideal Museum must fail in its object, because enough account is not taken of the practical impossibility of carrying out that advice; nor have the particular requirements of individual cases always received a sufficient amount of consideration. A Museum is a body with a large amount of inertia, and it necessarily takes time to set it in motion. We are told, for instance, that many of our specimens are not suitable for exhibition, or perhaps that the entire conception of our arrangement is radically wrong. We may be perfectly willing

[1] W. Smith, "Dictionary of Greek and Roman Antiquities," 3 Ed., II., 1891, Art "Museum."

to admit these impeachments, but we may at the same time be unable to take practical steps to remedy the defects. The unsuitable specimens are, after all, not without scientific value; but in the majority of cases the Museum cannot, under existing circumstances, protect them from deterioration except by keeping them in the exhibition galleries. The complete alteration of an existing system is a much more serious matter. It is only those who have actual experience of dealing with specimens who are likely to realise how much time is required for the satisfactory preparation of even a single exhibit. Any considerable alteration of a collection must accordingly be a gradual process, depending partly on the possibility of dealing with the old specimens and acquiring new ones, but to a very large extent on the amount of time which the staff of the Museum have for assimilating the new material.

I do not wish to convey the impression that I am opposed to the newer ideas of Museum-arrangement which have frequently been discussed by our Association. But I do feel that it is unreasonable to expect any sudden reversal of traditional policy.

The demand for an ideal Museum is sometimes made in such a way as to suggest that it is desirable to admit only a small number of stereotyped patterns, one of which is appropriate for a national collection, while others are to serve as the models for provincial Museums. Now it appears to me that it is not possible to expect every Museum to conform to one of these types; nor, indeed, is it altogether to be wished. There is hardly any quality which one is more inclined to admire in a Museum than that of having a character of its own. This individuality may be the result of the particular tastes of former Curators, or it may have been largely due to the labours of local workers who have enriched the collection with the objects in which they have taken special interest. This is conspicuously the case with the Norwich Museum.

When a Museum has once acquired some distinctive character of its own, which is in itself desirable, it seems to me that no effort should be spared to maintain and develop that character, so far as can be done without prejudice to the other objects for which the institution exists. No worker on

the Birds of Prey could afford to neglect the Norwich collection, and this Museum can benefit Ornithological science in no better way than by keeping that collection up to the highest level in its power.

In this connexion I may perhaps venture on dangerous ground, and I cannot expect that my remarks will command universal assent. I refer to the vexed question of type-specimens, or specimens on which new species have been founded. There are many Zoologists who wish to see all type-specimens placed in national collections. But it appears to me that if another Museum has established any sort of pre-eminence in a particular department, that Museum is morally entitled to accumulate as many type-specimens within those limits as can be got together. Anyone who undertakes the systematic study of a group of animals will probably feel the necessity of examining the collections of those Museums in which it is best represented. It may thus be maintained that type-specimens are advantageously placed when they form part of a really first-rate collection, whether that collection happens to be in a National Museum or not.

Perhaps I may next be permitted to indicate one or two of the directions in which I think that advances in Museum-arrangement may well be made, and incidentally to point out some of the principles which I think ought to influence those who have the charge of Museums. I must crave your indulgence for repeating what has often been said before, and is perfectly well known to most of you.

In speaking on these subjects, I do not propose to go outside my own somewhat limited experience. If in what follows I appear to confine my remarks too exclusively to Zoological collections, it is not because I ignore the fact that the Association takes cognizance of all kinds of Museums, but because I think it is better to leave to others the duty of speaking on subjects with which I am myself not directly concerned.

The majority of Museum Curators have to think principally of the general public, whose right to be considered is partly based on the fact that it is from them that the income of the Museum is derived. In the case of our Museums at Cambridge

we are able to arrange the collections entirely for the sake of serious students of the subject, and we can thus regard the Museum exclusively as an institution which has the object of facilitating study and research. This privilege, which is one to be highly valued, may perhaps induce me to take a one-sided view of the general subject.

In any Museum which is intended to attract those who are not serious students, it is obvious that a certain number of the exhibits must be of an attractive or popular character. An interest in Nature has to be stimulated by well chosen specimens, and in a certain number of cases, an interest thus stimulated may develop into serious study. The dwellers in large towns, who have but few opportunities of coming into personal contact with natural objects, are specially in need of the education which a Museum can afford. But in many cases the specimens exhibited fail to tell any connected story; and this is a direction in which improvements may often be made.

One of the objects which I think that a Zoological Museum ought to keep in view is that of illustrating and explaining the elements of the classification of the Animal Kingdom. For this purpose the labels are hardly less important than the specimens themselves. I do not believe in an indiscriminate use of long descriptions or explanations. I think that it is very easy to try to convey too much information by means of a label; and in particular I think that descriptive labels should be kept as short as possible. But something of this kind i essential if it is intended to explain the outlines of classification. What proportion of educated people could state the essential differences between a Fish and an Amphibian, or between an Amphibian and a Reptile? It is true that information of this kind can readily be obtained from books, but most of the visitors to an ordinary Museum do not study treatises on Zoology, though some of them might be induced to notice the main facts if presented to them in a short and simple form side by side with well selected specimens.

The characters on which classification is based are often internal characters; and it is accordingly important to have a series of preparations to illustrate some of the structural

features of animals. It may be objected that preparations of this kind are too lacking in attractiveness to form suitable exhibits in a popular Museum. But anyone who takes the trouble to visit the Entrance Hall of the Natural History Departments of the British Museum may easily convince himself that well made preparations illustrating the osteological or anatomical characters of animals may be objects of real artistic merit, well qualified to stimulate an interest in science.

It is in the lower groups of the animal kingdom that many Museums are the most defective. The classification of the Invertebrates in particular is often hopelessly out of date, and absolutely misleading. The specimens themselves are commonly entirely inadequate, and many groups of Invertebrates are not in any way illustrated. A group like the Mollusca, for instance, may be represented merely by a number of shells, as if the animals which constructed those shells were of no importance. Not only is it of general interest to give some idea of the architects which have constructed the shells, but there is hardly any group in the animal kingdom in which the illustration of classification more requires spirit-specimens. The scientific names commonly adopted for many of the divisions of the Mollusca are a sufficient proof of this statement.

Some of the groups of Invertebrates deserve special prominence on account of their practical importance. Most Museums include in their collections a certain number of the Corals whose agency in the formation of reefs and islands is of the most stupendous nature. But comparatively few Museums are in a position to demonstrate scientifically that reef-forming Corals are found among several of the large divisions of the Coelenterata, and that the majority of Coral-animals are creatures like Sea-anemones, in opposition to the popular belief that a Coral is an "insect."

The several groups of parasitic worms are barely represented in many Museums, but the economic importance of many of them well entitles them to a place in a popular Museum. The life-histories of many of these animals, and the devices by which they pass from one host to another are of absorbing interest; and although some of the stages are too

small to admit of being satisfactorily exhibited, drawings may be used to supplement the specimens which it is possible to shew. The study of the internal parasites which attack Man is of the greatest importance to members of the medical profession, and may be of profound concern to their patients. The farmer may be a sufferer to an enormous extent by the depredation of such parasites as the Liver-fluke (*Fasciola hepatica*), or the Tape Worm (*Taenia cœnurus*) which inhabits the brain of domestic animals, or the Nematode (*Trichina spiralis*) which encysts in their muscles, or those other Nematodes which attack his crops. An exhibit illustrating the life-histories of any of these pests may be of service in suggesting the possibilities of remedial measures. Thus anyone who realizes that the intermediate host of a Liver-fluke is an aquatic Mollusc would naturally remove any flocks which might be suffering from the attacks of that parasite from damp meadows.

Our associations with the parasitic worms are usually of an unpleasant nature, if one puts on one side the extraordinary interest which attaches to the study of the modifications by which they are enabled to hold their own in the struggle for existence. It is with some relief that we notice that they are not always prejudicial to ourselves; and the non-scientific visitor may derive some satisfaction from the knowledge that the production of pearls by certain Molluscs appears to be due to the existence in the tissues of the Mollusc of some of the earlier, microscopic stages in the life-history of certain parasitic worms.

Even in a popular group like the Insects, the exhibits are not always an adequate representation. The phenomena of variation are capable of being well illustrated by means of examples taken from Insects. Some of the most striking cases are those which have recently been obtained by breeding experiments made with certain African Lepidoptera. The results of these experiments are calculated to shake our confidence in the validity of many of the species which have been described, so great are the differences which may occur between the Insects reared from the eggs of the same parents. The illustration of protective resemblances by means of Insects has

not been neglected in many Museums, but the metamorphoses of Insects are seldom really well shewn, even in collections which devote much space to this group.

The practical importance of economic Entomology is a reason for devoting attention to this branch of the subject. Certain Insects, such as Mosquitos and Tsetse Flies, as well as some of the Ticks, have recently acquired immense importance, since it has been shewn that they are the carriers of diseases deadly to Man or to domestic animals.

The illustration of the structural features of animals naturally involves some knowledge of the use of reagents. The art of preserving animals in spirit has made great progress within recent years, and it is now possible to exhibit even the most delicate animals in a condition which is not entirely a caricature of their appearance when alive. We are unfortunately still unable to show many animals in their natural colours, but the illustration of their form and of their attitudes when alive is no longer impossible. This art cannot be learned without a certain amount of experience. It is not only a question of applying well-ascertained principles, but the methods employed have to be varied for different animals for no other reason than because what is effective in one case is not found by experience to do equally well in others. The greatest advances which have recently been made in the preservation of delicate animals are those which were introduced at the zoological station at Naples, owing to the skill and ingenuity of Cavaliere S. Lo Bianco, whose methods are now accessible to anyone who chooses to use them.[1]

A year or two ago it was my good fortune to have the assistance, in the Cambridge Museum of Zoology, of the late Mr. J. S. Budgett, who early in the present year fell a victim to tropical disease contracted in Nigeria during the course of a scientific expedition to that part of Africa during the preceding year. Mr. Budgett had an unusual aptitude for

[1] Lo Bianco, S., "Metodi usati nella Stazione Zoologica per la conservazione degli animali marini," *Mitth. Zool. Stat. Neapel*, ix., 1889-1891, p. 435. The paper is translated into French in *Bull. Sci. France et Belgique* xxiii. (=4e.Sér.,II.), 1891, p. 100; and into English, with additions by E. O. Hovey, in *Bul U.S. Nat. Mus.*, No. 39, Part M, 1899.

mounting spirit preparations, his success being due to a remarkable capacity for devising means of overcoming difficulties of technique, and for applying those means with great skill, his results being controlled by a fine artistic sense. Many of the methods which we now employ at Cambridge are due to Mr. Budgett's ingenuity; and among them one of the most successful has been a method of placing labels inside the jar instead of on the outside. The particular advantage of this method is that it avoids the discoloration of the labels, whether varnished or unvarnished, which is sure to take place with the ordinary system of labelling, whether from the dust in the air or from a leakage of spirit which flows down the outside of the bottle. I am able to show you one of these preparations, from which you will see that the labels have been printed on tissue-paper, which, when affixed to opal glass, suggests the impression that the label has been printed directly on the glass. Mr. Budgett employed this method with great success in illustrating the Geographical Distribution of a genus, family or larger group. Small maps are printed in outline on tissue-paper, and the Distribution is indicated by means of vermilion paint.

The preparation illustrates another point which seems to me of importance in a teaching collection. The systematic position of the specimen is indicated by means of the head-lines. The illustration of classification is greatly facilitated in this way, and the specimen acquires additional educational value. It is unreasonable to expect students to know by name all the genera which it may be advisable to display in a Museum; but he is more likely to have some acquaintance with the family, order or class, which is illustrated by a given specimen.

My remarks with regard to spirit-preparations have so far referred to Zoological collections only. We have in Cambridge a striking illustration of the advantage of employing similar methods in Botany. A collection of Botanical specimens in spirit was commenced some twenty years ago, by Mr. Walter Gardiner and Mr. M. C. Potter. These specimens are most admirably adapted for showing the structure of the parts of plants, such as flowers, fruits, bulbs, seedlings and the like.

This method, which to the best of my belief has been out little used in other Botanical Museums, deserves to be more widely known. The difficulty of making a collection interesting is greater in Botany than in Zoology, but the use of spirit-preparations adds immensely to the educational value of a Botanical Museum, notwithstanding the loss of the fugitive colours of the plants.

I come now to the accessory parts of a Museum, the necessity for which is not always sufficiently realized by those who plan the original installation, although I am well aware of the fact that this Association has often considered these matters.

One of the first requisites for a Museum is a sufficient allowance for the purchase of books, particularly if, as is so often the case, there is no other scientific Library in the same town. The Libraries of most of our Provincial Museums are entirely inadequate. Zoology, for instance, is a subject which is connected with an enormous literature; and the necessity for illustrating Monographs and other Memoirs with elaborate figures adds greatly to the expense of Zoological books as compared with others. The other subjects in which Museums are interested—Geology, Botany, Mineralogy, Ethnology, and so on—have also pressing claims. A substantial sum should in all cases be allotted to the Curator for the purchase of the books which are as necessary to him as tools are to a mechanic.

Another defect which is frequently observable in a Museum is a want of proper work-rooms. The cleaning and articulation of skeletons, and the preparation of spirit-specimens necessitate both space and apparatus. The preservation of material and the determination of species require something in the nature of a laboratory, equipped with a stock of reagents and apparatus for microscopical examination.

A third requisite is one which has repeatedly been insisted on by this Association, namely an ample amount of space for storage. The proportion of specimens which it is desirable to exhibit in the public galleries of a Museum should be but a small proportion of the amount of material which ought to be preserved in the stores. Even should a Museum restrict its collections to the local fauna and flora it would hardly be

possible—and it would certainly be undesirable—to exhibit every species found within the limits of its field of operations. Some of the species are too small to be available for public exhibition. In other cases an attempt to represent every species would result in a deplorable amount of want of variety in the exhibited collection. The illustration of the local fauna and flora is a most praiseworthy ideal for a provincial Museum to strive for: but the proper place for many of the specimens is the reserve-collection which is not exhibited to every casual visitor. A further reason for requiring a reserve-collection is for the purpose of comparison in determining the species of the specimens which it is desired to exhibit. Then in order to be able to select appropriate objects for exhibition it is necessary to have something to choose from. A Museum which confined itself absolutely to specimens which it proposed to exhibit would obtain far fewer prizes than one of a more acquisitive nature. But a more important reason for requiring a sufficient amount of store-room is that the Museum is the place where natural objects are preserved, not only for the amusement of people who visit the collection because they cannot think of anything better to do, but also for the sake of research. Animals are becoming extinct through the agency of man. A Museum which can preserve any of these for later generations will be doing good service to science. My friend, Dr. A. C. Haddon, has recently put forward a plea for the study of what he calls "vanishing knowledge." Museums can do not a little in the way of caring for specimens which it will be impossible to procure in the future.

The extent to which a Museum should permit itself to accumulate specimens is a very difficult question, to which I am not prepared to give a definite answer. In a paper by Professor Ant. Fritsch, recently published by the Association,[1] it is urged that a Provincial Museum should exclude everything which does not illustrate the fauna of a limited area, and that "World Museums, which endeavour to bring together everything from all countries, are becoming more and more impossible, and are not at all desirable." I am not prepared to assent completely to these propositions. There are many

[1] *Museums Journal*, III., p. 252.

scientific questions of interest to the general public which cannot be illustrated by the local fauna. The succession of Vertebrate life, as shown by the remarkable fossils discovered in North and South America for instance, cannot fail to be of educational value if properly exhibited in a Museum. The scientific interest of many types of animals not found in our our own country is so obvious that it is unnecessary to do more than mention the fact. It seems to me that the only practical consideration which can decide the question of what to accept and what to reject is the power of dealing satisfactorily with the material that is received. The really important thing appears to me to be able to dispose of everything in such a way that it can be found and referred to when occasion requires. Large collections of unsorted material, put away without system or order, are to be deprecated. It is impossible for a Curator to determine the species of every specimen he may receive—and this is a fact which the non-scientific public usually fail to understand ;—but it ought to be possible for him in each case to make a determination sufficiently approximate to allow the specimens to fall into some sort of order, which will permit of their being placed in such a position as to be found when wanted.

In dealing with store-specimens it is above everything necessary to have some practical system of arrangement. I should like to allude to two forms of case which I have found of special value in keeping such specimens. The first of these formed part of the subject of the Presidential Address delivered at Newcastle by Canon Norman.[1] It consists of a form of cabinet with interchangeable drawers adapted for the arrangement of comparatively small specimens, whether dry or in spirit, in tubes, glass-topped boxes or trays. I have had this system in use for some years, and I cannot speak too highly of its merits. The second is a system of arranging larger specimens in bottles, which was shewn to me some years ago by my friend Professor D'Arcy W. Thompson, who had introduced it into the Museum at University College, Dundee. A series of wall-cases, with glazed doors, are arranged with shelf-supports at uniform distances of two or

[1] Report of Sixth Annual Meeting, 1895, p. 25.

three inches. The shelves, which are made in the form of shallow trays, slide in and out on these supports, and can be arranged at any distances apart which are appropriate for the height of the bottles used. This system has great advantages over ordinary shelving. Since the shelf can be drawn forwards when it is desired to find a bottle, it can be made much deeper from back to front than in the case of a fixed shelf; and great economy in wall-space is the result.

There is one practice to which I should like to be permitted to allude as a source of grave errors in Museums. I refer to the insertion of information on the label which is inferred and is not really known from the history of the specimen. This is most commonly done with the locality. A specimen of uncertain history is received, and is determined as belonging to a particular species. The geographical range of the species is then ascertained from some Monograph or Catalogue of the group, and the information thus inferred is put on the label with nothing whatever to shew that it is not to be depended on. In a large proportion of these cases the information may be really correct, but in some of them the species has been wrongly determined in the first instance, and when the mistake is corrected at a subsequent period, there is a danger that it will be forgotten that there is nothing to authenticate the locality which appears on the label. The next stage in the series of mistakes is that the specimen is taken as the authority for the statement that the species has been recorded from some place where it has not actually been found. I am convinced that this practice is a most dangerous one, and there can be no doubt that it has been extensively adopted in many Museums.

In many cases where the history is not known it may, however, be advisable to give on the label certain particulars which can be inferred with a reasonable degree of probability. Thus the Geographical range may well be stated, so long as it is made clear that the information given refers to the *habitat* of the species and does not profess to be the locality of the specimen itself. Particulars of this kind may conveniently be given within square brackets. Accurate registration of all specimens received, with full information as to their history (or want of

history), is of the utmost importance as a means of avoiding the danger which I have indicated. The Register should be the ultimate court of appeal in any doubtful case; and the Catalogues should be drawn up in such a way as to give references to the original entries in the Register. It is unfortunate that the practice of keeping accurate records of specimens received was not adopted in the early days of Museums. I suppose that most of us have in our charge valuable specimens whose history is irretrievably lost. This has not necessarily been the fault of our predecessors. Private collectors do not as a rule keep very detailed records; and in the case of sportsmen's trophies and other specimens received after the death of the original collector, the scientific value of a donation or bequest may be seriously impaired by the want of such records.

The last topic on which I wish to touch is the museum as a place for research, whether carried on by the official staff or by scientific workers who are not responsible for the care of the specimens. It is by no means an easy matter for a conscientious Curator so to apportion his time as to carry on researches without interfering with his first duty, that of maintaining the collection in a state of efficiency. But I think that it ought to be realised by the Governing Bodies of Museums that their collections fail to satisfy one of the motives which justify their existence if they are not used when occasion requires for the advancement of knowledge. If this be admitted, it follows that the officials at the Museum should be persons who can sympathise with this object, and whose training fits them for taking their own share in the work. This end cannot be attained unless it is admitted that the post of Curator is one of considerable scientific importance, and should be paid for accordingly—a subject to which the attention of the Association was called in the Presidential Address delivered in London, in 1893, by the late Sir W. H. Flower. It is true that time devoted to research must to some extent be regarded as time taken away from the other duties of a Curator. But it by no means follows that the collections will necessarily suffer in consequence. The reputation of the Museum will increase in proportion to its activity in scientific work. When

the officials in charge are acknowledged authorities on some branch of their subject, the natural result is that the Museum becomes a centre to which specimens are sent by collectors who desire to see their treasures placed in some institution in which good use will be made of them. The collector is, moreover, not always an entirely disinterested person. One of his objects is to have his specimens reliably determined; and a Museum official who performs this service can often claim as the recompense for his labours some share of the collection which he has determined.

It is, however, not merely the staff of the Museum who are able to perform this work. Every facility should be given to workers of sufficient scientific capacity to pursue researches on specimens in the collection. This object can sometimes be best attained by the provision of adequate work-rooms in which such studies can be conducted on the spot; but there are also occasions on which it is desirable to allow the staff of the Museum to lend specimens to specialists at a distance. When this can be done, without prejudice to the objects lent, the value of the specimens is immensely increased by their accurate determination and by their being referred to in scientific literature.

I am fully conscious of the shortcomings of the Address which I have had the honour of delivering to you to-day, and I thank you for the patience which you have shewn in listening to it. Whether you agree with what I have said or not, I am confident that you will agree with me in this, that the maintenance and development of our Museums are objects which all Members of the Association may well be proud to share in promoting.

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British Association for the Advancement of Science.

SOUTHPORT, 1903

ADDRESS

TO THE

ZOOLOGICAL SECTION

BY

PROFESSOR SYDNEY J. HICKSON, M.A. D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

AT the last meeting of the British Association which was held in Southport, the President of Section D, Professor E. Ray Lankester, delivered an impressive address on the provision in this country for the advancement of Biological Science, in which he pointed out the very inadequate encouragement which existed at that time for those who, by education and inclination, were fitted to pursue original investigation in Zoology and Botany. Twenty years have passed since that Address was written, and yet we have to acknowledge that, notwithstanding the important part which our branch of Science has played in contributing to the sum of useful human knowledge during the last two decades, the progress made in the direction indicated by Professor Lankester is far from satisfactory. I do not propose in this Address to make any detailed statement of the number of posts in this country that are now open to zoologists, or of the amount of the present-day endowments for the encouragement of Zoological Science as compared with those of twenty years ago; but I wish to point out that neither in the older Universities of Oxford and Cambridge, nor in the Colleges and National Institutions situated in London, nor in the newer Universities and Colleges of the provinces, have any new posts been created or adequately endowed which enable the holder to devote a reasonable amount of his time to the pursuit of biological knowledge. It is true that there are a few more posts now than there were, in which a small stipend or salary is offered to young trained zoologists for their services as teachers of Elementary Biological Science to medical students and others; but the emoluments of such posts are so small, depending as they do, almost entirely, upon a share of the fees paid by the students, and the duties so arduous and prolonged, that they really offer very little inducement to the pursuit of continuous and systematic original research.

In one respect, however, we may notice and acknowledge, with gratitude, an improvement in our position. In the laboratory accommodation, both in our Universities and on the sea coast, we are a good deal better off than we were. Twenty years ago there was no biological laboratory on the whole of the long line of the British Coast. Now, thanks to the efforts made by biologists and their friends, we have at Plymouth an institution for the study of the marine fauna and flora under favourable conditions, and similar institutions at Port Erin in the Isle of Man, at Piel, at Millport, and at St. Andrews, and a provisional laboratory for the study of fishery problems at ~~Grimsby.~~ New laboratories for the study of Lowestoft zoology have also been built at Oxford, at Cambridge, at the University of Manchester, at Edinburgh University, and elsewhere, and I may add that a fine new laboratory is now in course of construction for the department of Zoology in the University of Liverpool.

These new institutions, however, only emphasise, they certainly do not amelio-

rate, the weakness of our position in having so little encouragement to offer to competent and well-trained men who wish to devote their lives to the advancement of Zoological Science. Moreover, I would point out that these institutions have been built and are being maintained almost entirely by funds supplied by private benefactors, or out of the inadequate resources of the Universities.

The Treasury has made a provisional grant of 1,000*l.* per annum towards the maintenance of the work done by the Marine Biological Association, and it may be supposed that a small share of the annual government grant made to the University Colleges and Scotch Universities goes to the support of the zoological departments; but, apart from this, there has been no increase in the support given to us from public funds.

If we were to compare our progress in the matter of the public appreciation of our science during the past two years with that in other countries, we should find that our position is by no means satisfactory. In Germany, France, Belgium, Holland, and more particularly in the United States of America, progress has been rapid and continuous. The number of persons in these countries who by the aid of university or public endowments are able to devote themselves to original work in zoology has considerably increased of late years, and the number of magnificently equipped institutions that have been built for their accommodation and convenience makes our efforts in the same direction appear very small.

It would not be difficult for me to bring facts and figures before you in support of these general statements; but my object is not so much to lament over the past and to mourn for the present position of our science in this country, as to suggest directions in which we may work together for its development and progress.

Upon one matter, however, I think we may congratulate ourselves. If the research done by English zoologists has not been as great in amount as it might have been, I think it may be truly said that we have fully maintained its standard as regards quality.

The contributions that have been made to the Science of Zoology by our countrymen during the past twenty years in general interest and in theoretical importance are of such a nature that any civilised race might well be proud of them, and I venture to say they compare favourably with those of any other country. I may remind you that the discovery and description of the Okapi, Cænolestes, Nyctotherus Rhabdopleura, Cephalodiscus, Limnocodium, and Pelagohydra, the rediscovery of Lepidosiren and Ctenoplana, the most important features of the development of Balanoglossus, Lepidosiren, Amphioxus, Peripatus Hatteria, and some of the Marsupialia, and that the discovery of the important character of the fauna of the deep seas involving the discovery of many new genera and species, were the work of British zoologists. Moreover, that the prolonged and painstaking investigations carried on in our laboratories have thrown much light upon the character and relations of cœlomic cavities, the homologies of the nephridia and genital ducts, and many other important morphological problems.

In the field of evolutionary theories we have done much important work in the study of the facts of protective and aggressive mimicry in insects, in the statistical estimation of variations, and in the experimental inquiry into the value of current theories of heredity.

The list is far from complete; but with such a record of good work done with the scanty means at our disposal there is no reason to suppose that the science is on the decline in this country, or that our countrymen are not as capable as any others of grasping the importance of biological problems and ultimately wresting from Nature the secrets that are hidden.

Whilst we may thus congratulate ourselves upon the achievements of the past and upon our strength and ability to carry on good work in the future, I cannot help feeling that the time has come for us to make a united effort to place before the general public of this country, and more particularly the educated and influential part of it, the disadvantages under which we suffer, and our need for help in the further development of our subject.

We have all realised that in this country, more than in any other that is called civilised, there prevails among all classes an extraordinary ignorance of the first

principles of biological science. It is this ignorance on the part of the general public, I believe, which prevents us from gaining that sympathy for our aims and that assistance for our efforts which we think is necessary not only for the reputation, but also for the welfare of our country. We must remember that the science of Natural History is as a closed book to most of those who after a public school and university education have attained to positions of trust and responsibility in the government of our country and our cities. Moreover, and this is perhaps the most serious aspect of the question, there are many who have gained a high position as men of science, and whose opinion is frequently quoted as authoritative on questions affecting science in general, who are more ignorant of the first principles of the science of biology than the Dutch schoolboy of fifteen years of age.

It appears to me, then, that it is of fundamental importance for the zoologists of this country to consider and report upon the necessity for the extension and improvement of the teaching of Natural History in our schools and colleges. We shall have to meet the objections that there is not time for Natural History in the school curricula, and that it is not a suitable subject for the instruction of boys and girls. These objections can be met, I believe, and overcome.

In many foreign countries Natural History is a compulsory school subject for all scholars. In Holland, for example, by the law of April 28, 1876, all scholars of the gymnasia during the first and second years devote two hours per week to the study of Natural History, and in the fifth and sixth years all students preparing for natural, mathematical, and medical sciences courses devote two hours per week to the science. In the superior middle-class schools one hour a week is devoted to the science in the first and second classes, and two hours per week in the remaining three years. If, therefore, time can be found in the middle and upper class schools for the study of Natural History in a country like Holland, where the general education is so excellent, surely time can be found for it here.

It is also a matter for general regret that some course of Elementary Biology is no longer compulsory for those who are proceeding to degrees in science in our universities, and I cannot help feeling that a very retrograde step was taken a few years ago by the authorities of the University of London, when Biology was made an optional subject in the Intermediate Examination for the degree of Bachelor of Science. We cannot expect to receive that sympathy in our pursuits and appreciation of our discoveries which we expect from our fellow-men of science if we tacitly admit that an elementary knowledge of the laws of living bodies is not a necessary part of the equipment of a man of scientific culture.

I think we must all admit that the time is ripe for a full discussion by biologists of the particular form of teaching and study which is most suitable for schools and elementary university examinations. It is a matter in which we are all interested; it is a matter affecting most intimately the interests of those who will be our pupils in the future, and we should be careful to see that no ill-considered or fantastic schemes of study are thrust upon the authorities by unauthorised persons at this very critical period in the educational history of our country.

There are other matters, however, which also demand our careful attention. The growth of our great cities and the improvement in our ideas of sanitation have brought forward as important problems for consideration the purity of the water-supply and the disposal of sewage. The municipal authorities at last realise that these problems can only be satisfactorily met by elaborate scientific investigation, and they have found that it is not only desirable for sanitary reasons, but also—and this has probably the greater weight—profitable to call in men of science for consultation and advice. At present, however, these problems are approached from only two points of view—the chemical and the bacteriological—the effect or effects of other organisms than bacteria upon the character of the sewage effluent and the purity of water for drinking purposes being, so far as I have observed, neglected. I was very much impressed with the fact that at the meeting of the Sanitary Institute last year in Manchester the speakers used the expression 'bacteriological examination' and 'biological examination' as if they were synonymous, and no mention was made either of the animals or plants which are

invariably present, and materially assist if they are not actually necessary for the maintenance of the most suitable balance of life in these waters. The time has come when an inquiry should be made of the organisms other than bacteria that are normally present both in the waters at the sewage works and in the large reservoirs which supply cities with drinking-water.

I may be allowed here to quote two cases that have recently come under my notice which will show the kind of work that might be done and the nature of the results which may be expected to follow such an inquiry.

Some years ago complaints were made that the water supplied by the borough of Burnley had an offensive smell. This smell was of such a nature that it was impossible to use the water for the manufacture of soda-water.

The smell was traced to the Hecknest reservoir, where the common water snail, *Limnæa peregra*, was present in enormous numbers. The problem to be solved was how to destroy or reduce the numbers of the Limnæa without interfering in other respects with the purity of the water. The authorities of the corporation asked the advice of a trained zoologist, who made certain recommendations which were adopted, and at a minimum cost the nuisance was abated, and during the six years that have elapsed has not recurred. I will not detain you with a full description of the cause and the cure of this particular pest, but I may say that the recommendations that were made were based on the knowledge of the life habits and reproduction of the Limnæa, and were therefore of a purely zoological character.

Two years ago the Chairman of the Water Committee of the Corporation of Manchester reported that the mains had become partially choked by the growth of an organism which he called a 'moss.' No less than 700 tons of this 'moss' were removed from the mains by a laborious and expensive process. It is not necessary for me to inform this Section that the organism was not a moss. It was probably not even a vegetable, but an animal belonging to one of the genera of fresh-water Polyzoa. In this case, however, so far as I am aware, not only were no steps taken to identify the organism, but no investigations were made to discover its origin or to prevent the return of the trouble in the future. I could give you several other examples which show that our ignorance of the general balance of animal and vegetable life in the large reservoirs is profound, and that a systematic inquiry conducted by competent persons would most certainly lead to knowledge which would be of great scientific importance, and in the long run remunerative to the community.

I do not think that we can expect that any one of the municipal authorities will feel justified in bearing the cost of such an investigation. The problems that one corporation has to face are very much the same as those that others have met; and each corporation hopes to profit by the successful and neglect the unsuccessful experiments of its neighbours. An investigation such as this, which is really for the benefit of the whole community, should be conducted by a central authority at the public expense.

The scientific investigation of the problems that are connected with the maintenance and extension of our sea fisheries is another matter that requires the very careful attention of the zoologists of the present day. The valuable work that has already been done by the officers of the British Marine Biological Association, the Lancashire Sea Fisheries Committee, the Scottish Fishery Board, and other bodies is of a nature sufficiently encouraging to justify us in asking for the necessary means and appliances for still further developments of the inquiry. There is, however, a great need for a free discussion by those who are competent to speak on the subject to determine and, if possible, to come to some conclusion upon the question of the best and most profitable lines that the inquiry should take in the immediate future, and the establishment of such co-operation as is necessary by the different authorities to prevent duplication where it is unnecessary, and simultaneous observations of similar phenomena on different parts of the coast when it is considered desirable. The report of the Committee on Ichthyological Research, 1902, has shown that there is already in this country a good deal of activity in various branches of investigation of the fisheries problems, but the authorities are not on all points in agreement as to the best plan or course to

pursue in the future. I cannot but hope that if some conference were held, at which those zoologists who have made a special study of these matters were present, the principal differences of opinion might be cleared up and a unanimous report presented to the authorities.

I have felt very strongly for some time past, and I know there are many of my colleagues who agree with me, that the zoologists of this country are under some disadvantage in not being provided with the necessary machinery for full discussion of matters which affect the welfare of the science as a whole. There are several societies which receive, discuss, and publish papers on various branches of zoological research, but they do not, and from the nature of their constitution cannot, give effective utterance to the general or unanimous opinion of professional zoologists on matters of their common interests. There is no society which all serious students and teachers of zoology feel is the one society which it is their duty and in their own interests to join. Some join the Zoological Society of London, others the Linnean Society, others, again, the Royal Microscopical, Entomological, or Malacological Societies, or some combination of two or more of them. There is no common ground on which we meet for the discussion of such subjects as those I have just mentioned in this Address. In the early days of the British Association this Section supplied the needs which we feel now. It was the Society, if I may call it such, which all the zoologists of the time made a special effort to attend. Important matters were fully discussed by the most competent authorities, and people felt that the prevalent opinion on any subject expressed by Section D was the prevalent opinion of men of science throughout the country.

In concluding this portion of my Address, I may express the hope that when the Association meets next year at Cambridge some steps may be taken to render the organisation which we already possess in connection with this Section more generally useful and more efficacious than it is at present.

In the opening sentences of my Address I used an expression which some of my hearers may have considered open to criticism. Let me take this opportunity of saying, then, that by using the expression 'useful human knowledge' I did not intend to express an opinion that there is any knowledge of the character that is expounded and discussed in these sections of the Association which can be called useless knowledge.

A distinction, however, is frequently drawn between knowledge that can be directly applied to the arts and crafts and knowledge which, on the face of it, appears to us at present to be only of general scientific interest. For example, in the award of the Exhibition (1851) Scholarships and Bursaries, the candidates must still give evidence of capacity for advancing science or its application by original research in some branch of science, *the extension of which is especially important to our national industries.* We can rejoice most cordially in the successful developments of the technical institutions in the country, we can heartily join hands with our colleagues in other sciences in urging upon the authorities the encouragement of those branches of science which have a direct bearing upon our industries, but we have a no less important duty to perform in claiming for those branches of sciences that have apparently no such direct application the needful sympathy and encouragement. I venture to say that at the time the Association last met in Southport no one would have ventured to predict that the study of the anatomy and life history of the Diptera, or the general biology of the minute sporozoa, would have any direct bearing upon the development of our industries. But to-day, by our knowledge of the mosquito Anopheles, and the sporozoan parasite it carries, we are in a position to destroy or ameliorate the malaria pest which has hindered the commercial development of so many of our colonies in tropical countries, and by encouraging the development of such countries we are assisting to a very material extent our home industries and the general trade of the country. In this, as in so many other cases, the benefit to industry and commerce has come from an unexpected quarter of the field of zoological research. Those who were working within the narrow limits of what is called applied science could have never discovered the facts which we now regard as of extreme

importance, however well equipped they were with laboratories and appliances and endowments for research.

It will be of very little profit to this country to endow munificently the technical institutions and those branches of science to which the adjective 'applied' is given, to build British 'Charlottenburgs,' and to attract by handsome salaries the most distinguished professors to the study of the application of science, if at the same time we starve and allow to sink into insignificance the fundamental sciences upon which the whole superstructure rests. It does not need a prophet to foretell that a great disaster will occur if we add story to story of our house of education without widening and broadening the basis upon which it rests.

Many of us, I am afraid, are too much inclined to believe that the intellectual portion of the community has at last awakened to the importance of the work in the fields of pure science, that the old prejudice against us who indulge what is called our harmless curiosity is dying out, and that our science is bound to receive a fair share of encouragement and attention in the progress of the modern developments of science and learning.

The distinction that is drawn between pure and applied science is, however, in danger of being broadened and deepened rather than diminished by the recent activity in the foundation of schools and colleges for technical instruction. There are, it is true, several eminent and distinguished persons who recognise the danger and do their best to avoid it, but this fact is not in itself sufficient to justify us in any relaxation of our efforts on behalf of the maintenance and development of those branches of the sciences which are usually supposed to have no direct or technical application.

In the wide field of zoological research there are many subjects now being investigated and discussed which, at present, seem to us to have but a remote bearing upon any practical problem of industry or medicine. Of all these subjects there are two which have excited during the past ten years extraordinary interest, and are from many points of view subjects of greatest possible importance. I refer to the subject of the natural variations of animals and plants, and the problem of the hereditary transmission of characters from generation to generation.

At present there appears to be some doubt whether the workers in these subjects are really agreed as to the general propositions of the problems, the definitions of the terms employed, and the standard of proof that is requisite in each step of progress. It is true that in most, if not in all, biological problems we are at the disadvantage of being unable to define or measure anything with the same mathematical accuracy that our friends, the chemists and physicists, are accustomed to. We cannot say for example that the chela of a particular species of crab is so many millimètres in length, in the manner the chemist determines the atomic weight of a new metal, as the length of the chela is found to vary within a certain range in all species that have been investigated; nor can we define such common expressions as a species, a variation, or even a cell with the same conciseness as a physicist defines the ohm, the volt, specific gravity, or the mechanical equivalent of heat. As a consequence it is not surprising that when our problems have been studied and a solution reached the resultant 'laws' exhibit so many exceptions that they are really not worthy to be called 'laws' at all. We may see the truth, but we see it as through a glass, darkly.

There is perhaps no word in the whole of our vocabulary which is used in so many different senses as the word 'variation,' and yet when it is used an attempt is only rarely made to define the sense in which it is employed.

When we study the adult progeny of a single pair of parents we notice that they differ from one another as regards any one particular character within a certain range. Thus the eight children of a single pair of human parents may vary in weight from, say, 130 lbs. to 200 lbs., and we may find that the average weight of the eight children is approximately the same as the average weight of the two parents. If parents and children were all of exactly the same weight—an impossible supposition—it would be said that they exhibited no variation in this respect, but, as they always do vary in weight, it is said that they exhibit

'variations' in weight. Now, such variations may be due partly to differences in the muscular training, the nourishment, the general health, and other post-natal causes; but it is assumed, and there are doubtless good reasons for the assumption, that if all these post-natal influences had been equal throughout life there would still remain variations in weight of lesser amplitude than is usual, but nevertheless substantial.

The variation of the adult in weight, therefore, is a compound quantity, partly due to the influence of external conditions upon the growing body, and partly due to a quality or character present at birth and usually supposed to be inherited with the germ-plasm from one or both parents. The former may be called the artificial part of the variation, or for brevity the artificial variation, and the latter the natural or inherited variation. In the character of weight in human beings there can be no doubt that artificial variation is predominant, the character being a very fluctuating one and liable to profound modification in the varying vicissitudes of civilised human life.

In the character of stature the artificial variation is probably much less predominant. The children of tall parents grow into tall men and women, however handicapped in early life by ill-health or insufficient nourishment, and the children of short parents remain short in adult life, however healthy and well fed in their youth. Nevertheless, he would be a bold man who would assert that the character of stature is uninfluenced by the environment, and that the short people would not have been taller had the conditions of their life in childhood been more favourable, or the tall people shorter if the conditions in their early life had been less favourable.

Finally, we have, in the colour of the iris, the shape of the ear, and the size of the teeth, characters which are usually considered to be unmodified by post-natal conditions, or at least so slightly modified by them that the differences observed in them may be regarded as almost pure natural variations. Now, if we turn our attention to characters such as weight, which we feel certain are influenced very profoundly by the environment, we might be tempted to exaggerate the importance of the environment in moulding or forming the characteristic features of the adult organism, as, in the opinion of many authorities, Lamarck did, and many of his followers are still doing. If, on the other hand, we confine our attention to such characters as the colour of the iris or the shape of the ear, we might be tempted to under-estimate the influence of the environment.

This brings us to the important question whether the characters of the adult that are due to the influence of the environment, and that part or degree of any character which is more or less modified by the conditions of the earlier stages of life are or are not transmitted by parents to their offspring. Time will not permit me to discuss this difficult problem here. Rightly or wrongly, I agree with those who maintain that acquired characters are not inherited, and I intend to assume for the purpose of the argument that follows that they are not inherited. I will also assume, and I must say that the facts seem to be conclusive in favour of this assumption, that the characters which are usually supposed not to be influenced, or to be only slightly influenced by, the environment are capable of transmission by heredity.

We have, then, in most variations a part that can be transmitted and a part that cannot be transmitted by heredity from parents to offspring, and we find in every plant and animal an enormous difference in the proportions of these two parts in different organs. It is not difficult to see the general reasons for these differences. It is clearly important that some organs should be plastic—*i.e.* capable of changing in form and size to meet the varying changes in the environment, and that others should remain relatively constant in spite of changes in the environment. Thus the shape and size of the branches of an oak in a plantation will vary enormously, according to the light and space they have for their development, whereas the anthers, the pistils and fruit, will be relatively constant in form and colour. It is clearly important for a chamæleon that the colour of its skin should vary according to the colour of its environment; but it is none the less important that the shape and muscular organisation of its tongue should remain relatively constant throughout life.

An essential point, however, for us to consider is whether there are any characters in animals or plants upon which the environment exercises no influence at all or exercises such a slight influence that it may be safely neglected. The method to adopt in order to settle this point would be to compare at a definite period of their lives the statistics of variation in a family or population which has been brought up under identical circumstances with a similar family or population at the same period of life which has been brought up under differing circumstances. If this were done we could determine with considerable accuracy the proportion of the variation of any character of the individuals that is due to the environment and that which is natural and inherited.

Unfortunately it is impossible to bring up a population under identical circumstances. If we take, for example, the individuals of a single hive of bees, which have the same parents, pass through the early stages of their development in cells which are almost identical in size and are regularly fed by the workers during the whole of their larval life, there is still a considerable probability that the individuals do not have a treatment which can, with any pretence to accuracy, be called identical. The food that is collected by the worker-bees frequently comes from varied sources or from flowers in different stages of their growth, and it is impossible to believe therefore that it has always identical nutritive properties; the larvæ are not of the same age, and seasonal changes may affect the larvæ differently, some being checked in the early stages of their development more than others.

But even if we could, with justice, assume that the conditions of life for the individual bees in a hive are identical from the time of hatching up to the time when the adult characters are assumed, there still remain two sets of variable conditions which must affect the development independently of the influences brought by the two parents in the germ-plasms.

In the egg of the bee there is a considerable quantity of yolk, and this yolk is the food material upon which the embryo is nourished throughout the earlier stages of its development. There is no evidence that the yolk in the eggs of this or of any other animal is constant either in quality or quantity. On the other hand, the extraordinary variations or abnormalities, as they are usually termed, which the embryologist meets with in the segmentation of the egg suggest that there are considerable differences in these respects between the eggs laid by a single parent in a single act of oviposition. Moreover, the manner in which the young eggs of the insects are nourished in the tubular oviduct before they are ready for fertilisation gives very little support to the view that the amount of yolk deposited in each egg is identical.

The second consideration under this heading is possibly of even greater importance. Vernon [1] has shown that the size and other characters of echinoderm larvæ vary very considerably according to the freshness or staleness of the conjugating ova and spermatozoa. For example, he found that when the fresh spermatozoa of Strongylocentrotus fertilised the eggs which had been kept eighteen hours of the same animal, the larvæ differed from the normal larvæ, $-17{\cdot}6$ in body length and -15 per cent. in arm length, and when the fresh eggs are fertilised by spermatozoa which had been kept eighteen hours the resulting larvæ differed from the normal by $+11$ per cent. in body length and by $-32{\cdot}8$ per cent. in arm length

This consideration is practically eliminated in the case of the ~~worker-bees~~ by dro
parthenogenesis, but it cannot be set aside in the case of the ~~drones~~ nor in the wor
cases of the broods of other animals which do not exhibit the phenomenon of parthenogenesis. A comparison of the curve of variation of some character, common to both, in drones and worker-bees from one hive would perhaps throw some light on the general importance of this character.

Before leaving this part of the subject, I must call attention to two results bearing upon it, obtained by De Vries in his botanical investigations, and related by him in his very important work entitled 'Die Mutationstheorie.' This ob-

[1] H. M. Vernon: 'The Relations between the Hybrid and Parent-forms of Echinoid Larvæ.' *Phil. Trans.* 1898, B. p. 465.

server found that the younger the seedling is the greater is the influence of external circumstances upon its adult characters, and in the second place that an even greater influence is exerted upon the characters of a plant by the external circumstances affecting the mother-plant. If these results hold good for animals as they do for plants, we should expect to find, then, that the external circumstances affecting the mother at the time she is maturing the eggs in her ovaries and the external circumstances affecting the embryo before and during the larval period are of far greater importance in affecting the curve of variation of the adults than are the external circumstances affecting the young in their period of adolescence. We must come to the conclusion, from these considerations, that the general variability of a brood or progeny of a single pair of parents must be very largely the effect of the varying conditions affecting the gametes from the earliest stages of their genesis in the gonophore, the fertilised ovum, and the early stages of development. We find, however, as I have already pointed out, that some characters are much more influenced by external circumstances than others. Weight and stature in human beings, for example, are probably much more influenced than the colour of the iris or the shape of the fingers. We may, indeed, recognise two kinds of characters, connected, of course, by a complete series of intermediate links, which may be called, for convenience sake, plastic characters and rigid characters.

Now, in some animals, the characters that are rigid are much more numerous than they are in others. For example, adult salmon or perch are much more variable in size and weight than adult herrings or mackerel; some species of butterflies are much more variable in the colour and pattern of their wings than other species; some species of birds are much more variable in their plumage than others are. Several other examples could be chosen to illustrate this point from the higher groups of animals; but I wish particularly to call your attention to several instances found in the Cœlenterata, because it was the special study of this group of animals that led to the train of thought I have ventured to put before you.

In all the sedentary forms of Cœlenterates the mouth is surrounded by a circlet of tentacles. These organs are used for catching and paralysing the prey and passing it to the mouth to be swallowed. They are also very delicate, and indeed the only specialised organs of sense performing a function similar to that of the feelers or antennæ of Arthropoda. There can be no exaggeration in saying, therefore, that they are of the utmost importance to the animal. In some groups of Cœlenterata, however, we find that they are fixed in number, but in others that they are variable.

In the Alcyonaria, for example, the number of tentacles of the adult polyp is eight. I have examined many thousands of polyps belonging to the suborders Stolonifera, Alcyonacea, Gorgonacea, and Pennatulacea, and I have not found a single example of an adult polyp with either more or less than eight tentacles. This is a character, then, which is remarkably well fixed in the Alcyonaria. It does not fluctuate at all. The tentacles of the Hydrozoa, and of many of the Zoantharia, on the other hand, fluctuate considerably in number. In some forms, such as Tubularia among the Hydroids, and Actinia among the Zoantharia, the number of tentacles is considerable, and it is not, perhaps, surprising to find variations in their number. But in many cases, when the number of tentacles is small, there is also frequent variation. In *Hydra viridis*, for example, the number of the tentacles is 6, 7, or 8, and more rarely 5 or 9.

Again, in the Alcyonaria, the number of mesenteries of the adult polyp is always eight; never more and never less.

In the Zoantharia, on the other hand, the number varies not only in different sub-orders and families, but even in different individuals of the same species from a single locality. Parker found, for example, that the number of non-directive mesenteries in the sea-anemone *Metridium marginatum*, collected at Newport, R.I., varied from four to ten pairs in those forms with the normal number (2) of directive mesenteries, and that there were further variations in the number of non-directive mesenteries in those forms with an abnormal number of directive mesenteries. In fact, of the 131 adult specimens collected, only 40 or about 33 per cent. exhibited the arrangement of mesenteries which is regarded as normal

for the species. On the other hand, Clubb found that of the specimens of another common sea-anemone, *Actinia equina*, only 4·24 per cent. showed variations from the normal mesenterial arrangement for the species. We have then, in these examples, a set of organs which are very variable in one genus (Metridium), much less variable in another (Actinia), and perfectly fixed or rigid in another series of genera (the Alcyonaria).

Passing on, now, to the character 'shape.' Not many years ago the systematic zoologists, who directed their attention to the sedentary Cœlenterates, based their specific diagnoses very largely on the shape of the colonies. Thus we have introduced such names as *Millepora alcicornis*, *M. ramosa*, *M. plicata*, *Madrepora cervicornis*, *M. prolifera*, *M. palmata*, *Alcyonium digitatum*, *A. palmatum*, &c. &c. Zoologists are now agreed, however, that the shape of these colonies is so variable that in most genera it is of very little value for the separation of species. In fact, I have elsewhere given reasons for holding the view that the widely distributed and very variable genus Millepora is represented by only one true species. But what is true for most sedentary Cœlenterates is not true for all colonial Cœlenterates. In most of the genera and species of Pennatulida, for instance, the shape of any one individual of a species is almost identical with that of any other. A *Funiculina quadrangularis*, from the west coast of Scotland, is similar in shape to one of the same species from the coast of Norway. A *Pennatula murrayi*, from the reefs of ~~Funafuti~~, is similar in shape Suvad to one from Ceram. In other words, the character 'shape' is extremely plastic in Millepora and Madrepora, but very slightly plastic or almost rigid in Pennatula and Funiculina.

This difference in the plasticity of the character 'shape' in Millepora and the Pennatulids must be associated with the fact that the young Millepora colony is unable to move from the spot where the larva settles, whereas the Pennatulid is capable of moving from place to place throughout life. The Millepora colony must either accommodate itself to the environment in which it begins life or perish, but the young Pennatulid can, within certain limits, travel to the environment that suits itself.

The shape of a growing coral or sedentary Alcyonarian on a reef must accommodate itself to the depth of water, the position of neighbouring zoophytes to itself, the direction of the tides, and other influences; and such a power of accommodation is essential for the species in the struggle for existence on the coral reef. But in the case of the Pennatulid, the natural or normal shape is adapted to a less variable series of environmental conditions, and it has sufficient power of movement to shift itself into localities where the environment is suitable for it. In other words, the power of movement is associated with a loss of plasticity of the character 'shape.'

But the growth of corals may be affected in other ways. A great many of these forms of life harbour a small fauna of epizoic crustacea, mollusca, and worms, and the ramification or surface is often affected by these in a remarkable way. I have elsewhere pointed out that the character of certain specimens of Millepora, which is known as verrucose, is due to a modification of the growth round epizoic barnacles. Semper has shown that the curious cage-like growths seen on the branches of Seriatopora and Pocillopora are galls produced by the action of certain species of crabs. In a recent paper I have also given reasons for believing that the tubular character of the stem and some of the branches of the genus Solenocaulon is due to the action of certain crustacea belonging to the family Alpheidæ, and that when these Alpheids are not present the form with a solid stem hitherto known as the genus Leucoella is produced.

But whilst some genera of corals and Alcyonaria are plastic in this way, others are not. These coral galls may be found on the Milleporas and Madreporas of a certain portion of a reef and be absent from all the other genera of neighbouring corals. The crab-galls that are found so commonly and in such abundance upon Pocilloporas and Seriatoporas in certain parts of the Pacific and elsewhere are found only in cases of extreme rarity in other corals.

Many other cases could be given to show that in some genera the cœnenchym

is remarkably plastic or accommodating to these epizoites, whereas in others it is resistent and rigid.

The size and shape of the spicules have been taken as characters for the determination of the species of Alcyonaria. It is true that in some species the spicules are remarkably constant in size and shape, but in others they are extremely variable. The remarkable torch-like spicules of the cœnenchym of *Eunicella papillosa*, the club-shaped spicules of Acrophytum, and the needle-shaped spicules of many species of Pennatulids are remarkably constant in size and shape, but in Sarcophytum, the new genus Sclerophytum, Siphonogorgia, Spongodes, and a great many others, the size and shape of the spicules are extraordinarily variable. In the matter of colour, too, we find the same thing. The genera Tubipora and Heliopora are widely distributed in the shallow waters of the tropical seas and are very variable in many of their characters, and yet there is not a single specimen of Tubipora known that is not red, nor a single specimen of Heliopora that is not blue. The same may be said for several other species. On the other hand, many species of Alcyonaria are extremely variable in colour. Thus, *Muricea chamæleon* is, according to Von Koch, sometimes yellow, sometimes red, and in some cases specimens show both red and yellow branches. The specimens of *Melitodes dichotoma* in Cape waters are sometimes red and sometimes yellow. In a small species of Melitodes from the Maldive Archipelago there is a very remarkable degree of variation in colour both in the nodes and internodes, the details of which I have briefly described in vol. ii. of Mr. Gardiner's Results. In the genus Chironephthya, also from the same Archipelago, the variations in colour are very remarkable, the spicules of the general cœnenchym showing various shades of red, pink, yellow, and orange, and the crown and points purple, yellow, and orange colours which sometimes agree, but usually do not agree, with the general colour of the cœnenchym. The variability of the genus is particularly interesting, as in Siphonogorgia, the genus which comes nearest to it, and is, in fact, difficult to separate from it, the colour of the cœnenchym is almost invariably red.

To summarise this knowledge of variability in the Cœlenterata we may say that we find either extreme plasticity or remarkable rigidity in many of their most important characters. Such important and essential organs as the tentacles, stomodæum, mesenteries, &c., are in some groups very variable indeed, and in others as stationary or fixed; we find the same with organs such as the spicules of Alcyonaria, which are, so far as we can judge, of less essential importance, and in characters, such as colour, which must be, in the sedentary forms at least, of minor importance.

If we compare this with what we find in the higher groups of animals we observe a great contrast. In fishes, to take an example at random, we may find that in such characters as the size and weight of the adults, there may be great or considerable variability, but in the essential organs, such as the heart, brain, and stomach, there is almost complete rigidity. I do not mean by using the expression 'rigidity' to imply that minor variations in size and shape do not occur, but that major variations, such as a doubling of the stomach, a bifurcation of the cerebral hemispheres or other variations, which it would be considered grotesque to suggest even, do not and cannot occur. But even in minor characters, such as colour, the possible range of variation in a fish is far less than in Cœlenterates. We may find in the mackerel, for example, that individuals differ in the shade and range of the green pigment, but we do not find in any species of fish that some individuals are red, some yellow, some purple, &c.

The contrast in this respect between the Cœlenterate and the fish must be associated with their different degree of complexity of structure. In a complicated organisation such as that of a fish, the brain, heart, and stomach must mutually work together; they must be co-ordinated in form and action. Any profound variation or abnormality of one would interfere with the action of the others and would therefore be incompatible with continued existence. In the Cœlenterate, however, the doubling of the siphonoglyph, the duplication or quadruplication of the mesenteries does not, in some cases, interfere materially with

the action of the other organs of the body. If we were to alter the size or shape of some part of a simple machine it might be able still to do its work the better or the worse for the change, but if we were to alter the corresponding part of a complicated machine it would probably throw it out of gear and prevent any work being done at all.

From this consideration we gather that in the process of the evolution of the higher forms of life there has been a gradual diminution in the range of variation of the different characters of the body, a gradual diminution of the response of these characters to changes of the environment. Characters which, in the early stages of evolution, were probably plastic become rigid.

The gradual evolution of the power of co-ordinated movement has been undoubtedly accompanied by a loss in the variability of the shape of the body, the gradual evolution of a blood vascular system and nervous system has led to a loss of variability in the alimentary canal with which they are associated. In the majority of cases, however, we are much too ignorant of the facts of the co-ordination of the parts of the body or of the co-ordination of any one part to the environment to be able to frame an hypothesis as to why any one character has become rigid. It is difficult to see the reason why the number of the tentacles and mesenteries in Alcyonian polyps has become fixed at eight, while in other Cœlenterates these characters are so variable, or why the colour of Tubipora is always red, and of Melitodes variable.

The study of species, however, teaches us that, in all cases, except perhaps in some examples of degeneration, the plastic condition of the characters was antecedent to the rigid, that in the earlier stages of evolution the condition of extreme plasticity and ready response to changing external conditions were necessary for the survival of the species; and that in the later stages, when special adaptations to special circumstances were developed, a certain rigidity or indifference to changing external conditions was equally necessary for its survival.

Now, the study of the various orders of Cœlenterates conveys a very strong impression that the part played by the environment in the production of the variations of the adult is much greater in proportion than it is in the higher groups of animals. It is true that direct proof of this is wanting. Such a direct proof can only be obtained by experiments in rearing and breeding under varying conditions, and there are at present many serious difficulties to overcome before experiments of this nature can be satisfactorily made.

Nevertheless, the circumstantial evidence in favour of the truth of this impression is, to my mind, so strong that we are justified in considering its bearing upon the general question. It is quite impossible for me on this occasion to set before you at all adequately the general nature of this circumstantial evidence. To do so would involve statements concerning the actual variations of a large number of species already observed in one locality and in several widely distributed localities, with a discussion of the possible direct influence of the conditions of such localities, so far as they are known, upon each of the principal variations. Such statements would necessarily be of such a special and technical kind that, even if time permitted me to make them, they would not be suitable for an Address of this character. I may be permitted to say, however, that I am collecting and preparing the evidence for publication on this point at a later date. There can be no doubt, however, from the evidence I have already submitted to you in part, that some species are far more influenced by changes in the environment, or, to simplify the expression, are far more plastic than others; and we may conclude that in the evolution of other groups of animals the earlier forms were far more plastic than their modern descendants. In the earlier stages of evolution there must have been in the first instance a lessening of the power of change in structure according to change of environment. The fixity or rigidity of certain characters thus produced enabled a more elaborate co-ordination both in form and action to occur between one set of organs and another. It permitted a further localisation and specialisation of functions, or, in other words, further differentiation of the animal tissues.

Accompanying this differentiation there was a loss in the power of regeneration.

As Trembley showed many years ago, a Hydra can be cut into many pieces, and each by the regeneration of the parts that are missing will give rise to a complete individual. The Earthworm can, when cut in half, regenerate a new tail but not a new head region. An Arthropod dies when cut in half, but has the power of regenerating new appendages in place of those that are lost. But in Vertebrates there is very little power of regenerating new appendages, and the general powers of regenerating new parts are reduced to a minimum.

Now, whether the loss in the plasticity of characters was the cause of the loss in the power of regeneration of lost parts, or the loss in the powers of regeneration was the cause of the loss of plasticity, is a problem upon which I do not feel we are competent to express a definite opinion; but that the two series of phenomena are intimately associated is, I believe, a generalisation that is worth a good deal of further thought and study.

In Vertebrates, however, although the power of regeneration of lost parts is at a minimum, it is not by any means entirely wanting. The muscles, nerves, epithelia, and other tissues, are able to repair injuries caused by accident and disease. And similarly, although the power of response of various organs to the changes of external conditions in Vertebrates is very much diminished as compared with that in the lower groups of the animal kingdom, it still remains in an appreciable degree. Whether the curves of variation of the so-called fluctuating characters of Vertebrates represent simply or solely the influence of the environment on the organism cannot at present be determined with any degree of certainty; but it appears to me that zoological evidence, confirmed as it is in such a remarkable way by the recent researches of the botanists, points very strongly to the conclusion that the major part of each such curve is, after all, but an expression of the influence of the environment. In venturing to put before you these considerations, I am quite conscious of the vastness and complexity of the problems involved and of the many omissions and imperfections which a short Address of this kind must contain. Not the least of these omissions is that of any reference to the distinction that might be drawn between continuous and discontinuous variations in the simpler forms of life. This is a matter, however, which involves so many interesting and important questions that I have felt it to be beyond the scope of my Address to-day.

We are still in need of further systematic knowledge of the widely distributed species of Cœlenterates; we want to be able to form a more definite opinion than we can at present upon the value of specific distinctions, and we need still further observations and descriptions of the phenomena of irregular facies, abnormal growths, and meristic variations. But more important still is the need of further researches in the field of experimental morphology.

When we have accumulated further knowledge on these lines in a group of animals such as the Cœlenterata, of relative simple organisation, we shall be in a better position than we are now to deal with the problems of heredity and variation in the far more complicated groups of Arthropoda and Vertebrates.

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AN

INAUGURAL LECTURE

DELIVERED IN THE MEDICAL SCHOOL
CAMBRIDGE
ON JANUARY 29, 1904

BY

F. HOWARD MARSH, M.A., F.R.C.S.
PROFESSOR OF SURGERY

CAMBRIDGE:
PRINTED AT THE UNIVERSITY PRESS.
1904

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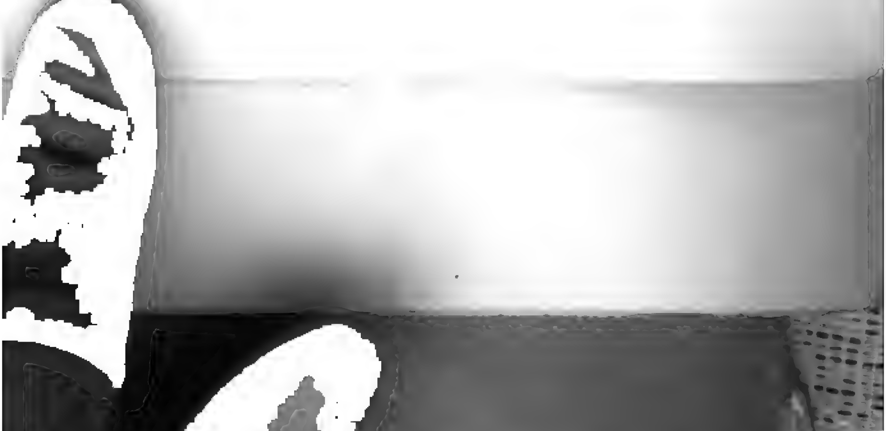

MR VICE-CHANCELLOR,

I must begin by assuring you that I am fully conscious of the honour of the position I occupy, as Professor of Surgery in your University. I am conscious, also, that as the honour is great, so likewise is the responsibility. This responsibility, which to-day weighs heavily upon me, I shall henceforth at all times bear in mind, and endeavour to meet it to the full.

Having now joined your Medical School, and having in the future to take part in its work, I am able to reflect, with more complacency than was possible when I was attached to, in a sense, a rival institution, that this department of the University of Cambridge occupies a position which you may well contemplate with genuine satisfaction. The

number of students shows that your medical school is highly successful, and this success depends on the fact, that the various departments are presided over, and personally conducted, by great masters who have themselves, in their generation, considerably advanced the sciences which they severally represent. Anatomy is taught by one who has long cultivated and is deeply versed in many other fields of research, and who is able to expand and enrich his particular subject by making the neighbouring sciences largely contribute to it. In your late Professor of Physiology you had one who has long and easily held the first place; whose name is known and revered wherever physiology is taught, and whose position in the world of science outside, and in the public eye, is one of the highest importance and distinction. As you yourself, Sir, have said, "It is not easy to express the greatness of the debt

which the study of biological science among us owes to Sir Michael Foster, both before, and after his election to the Chair in 1883." To follow him, and to adequately continue the work which he has so long carried out, must be indeed a difficult task; but every-one concerned feels that in Professor Langley, —at first his pupil, and subsequently his most able and accomplished fellow-worker,— a worthy successor has been found. In your Professor of Pathology you have secured, in Professor Sims Woodhead, a man whose departure from London was regarded as a very heavy loss. This, I am able to state, from personal knowledge, was especially felt to be the case in the Royal Colleges of Physicians and Surgeons, where his services in the conjoint Science Laboratories were much valued and appreciated.

Your Regius Professor of Medicine has for many years been my intimate friend. As

a man of wide scientific culture, as a learned physician of unusually wide experience, as a teacher, and, as I may be permitted to add, in his whole personality, he occupies a foremost place; and it must be gratifying to him to observe that his System of Medicine has been an unqualified success. The constant and earnest work of Dr Bradbury, Downing Professor of Medicine, is well known to all. There are many others among you, whose names are household words. Of these I cannot refrain from mentioning Dr Donald MacAlister and Mr Shipley, whose co-operation and unstinted labour, and devotion, are conspicuous factors in your success.

Indeed Cambridge occupies a position as a Medical School which has excited the envy —always, however, mixed with admiration— of us all, whether in London, or elsewhere. And for two principal reasons. First, because as I have said, the staff is so distinguished,

and secondly, because, as if by magic, there has sprung up a mass of new buildings allotted to your various departments which, for convenience and fitness, are unsurpassed, and I might even, I believe, truthfully say, unrivalled in this country.

Now, anyone who has studied the subject of education, not necessarily in detail, but in its broader lines, must be struck by recent events, in this renowned University. The closing half of the last century must always remain a landmark in the intellectual progress of our race; for during these years there was a new departure, which was to issue in what was scarcely less than a transformation. Until that time intellectual activity here, and elsewhere, had been largely expended in the field of literature, and in the cultivation of general learning. The leaders were poets, pure mathematicians, historians, and great novelists. Then we had our great ecclesiastics, our

renowned parliamentary orators, and our illustrious judges,—all as examples of the strength which culture adds to mere intellectual powers; while, in the imperishable literature of Greece, and Rome, was a field which offered ample opportunities for the acquirement of culture in its highest forms, and which afforded, also, welcome relief and refreshment to many a one who was wearied by the anxieties and worries of life.

But, in the years to which I have referred, another world was more and more distinctly disclosed, and as it gradually emerged from the nebulous stage, and took specific form, every observer felt that a new order of things was being established. The new power which now assumed its sway, which was so profoundly to modify human progress, and, under whose beneficent influence, events as yet undreamt of, and inconceivable till they stood revealed, and established as matters of fact, broke in upon us on every side. And here

was the advent of modern science, which, like light itself, was to be all pervading and vivifying, promoting, and controlling. It touched everything with creative power. Nothing was left unaffected by it.

Now these two departments of intellectual activity, to which I have referred, the *literae humaniores* on the one hand, and modern science on the other, are no natural foes whose interests are opposed, but influences which are intimately related, and destined to travel side by side, yet with a gradual approach, until, blending in closest assimilation, and interaction, they shall at length embody the highest developments of human achievement. They are in fact but

> ...parts of one stupendous whole
> Whose body Nature is, and God the soul.

Now of all human institutions it was clear that those most closely concerned in these events were the Universities—among

the foremost of which stood the University of Cambridge. And here, obviously, the new order demanded early recognition, and a fostering home. By the new order I mean the intimate association of modern science, with the culture, and elevation of the human mind, and character, which naturally springs from the pursuit of the *literae humaniores.* It is in the just balance between these two departments that true progress lies.

If we glance at this University to-day we cannot fail to see, and seeing, to be unfeignedly satisfied, that this consummation has been safely, and in good time, achieved. The renown of this great University as, in the words of your statutes, "a place of education, religious learning, and research," is not only as splendid as it has ever been, but it constantly increases.

I have been among you for so short a time, that there are many things of which it would be presumptuous of me to speak. But

this much I will say, without fear of contradiction. I am perfectly certain that no one who has not looked into the matter for himself can have any idea of the amount of work that is being at present done at Cambridge, or of the activity, the vigour, and the success with which your numerous departments are being developed and maintained. Some may, and I believe do, ignorantly think that the older Universities are half-asleep; that they are living on their reputation, and droning away their time. A short residence here has completely demonstrated the fact to me—of which I must own I was not previously quite fully aware—that the amount of work which is done in many of your departments is constantly so heavy and exacting as to tax the endurance of strong and determined men. Idlers no doubt there are, but they are few in number, and opposed to the spirit which pervades your University life. On the scientific side, we find that every department

is assiduously followed, from astronomy, in which your distinguished professor and delightful exponent, Sir Robert Ball, is confronted with distances which even he cannot always quite accurately measure, and with periods of time in which millions of years appear more brief than "fly ephemeral, which has its day," down to those sections of pathology, which are concerned with a study of objects, so minute, that their presence is revealed only by the most elaborate methods of research.

These two blood relations, learning, which is power and comeliness, and science, which in the keen competition of our daily life will, with many, henceforth mean daily bread, must work heartily together for mutual support and progress.

Now the development—on the science side of the University—of your Medical School was largely due, in its inception and early progress, to my predecessor in this

Chair. Later on, several of his colleagues who shared his views, and notable among these was Sir Michael Foster, worked heartily with him. Sir George Humphry was well suited for this new departure, which was to reach such important developments. He was a man whose cast of mind was essentially scientific. He constantly worked as if he wanted to know. And with this natural mental attitude he combined sleepless perseverance and quite remarkable shrewdness. He would take nothing for granted, especially statements made by other people. Discussion, even controversy, he dearly loved. He was the best I have ever known at holding his own, and that not only with the keenest tenacity, but with an ingenuity of resource which must, in some cases, have provoked his opponent to protest

> An I thought he had been...so cunning in fence
> I'd have seen him damn'd ere I had challenged him.

Now here are the elements of which men who achieve great results are made. Imagination, or in other words, the power of conception, as, for example, the power by which Cecil Rhodes conceived great imperial ideas, the mental endowments necessary for the work in hand, and the abiding tenacity which ignores all opposition, and goes on steadily, even, I had almost said, stealthily, upon its way. We used to watch Humphry, not only with interest, but with amusement, and think that his biography was summed up in the words of a popular advertisement, "He won't be happy till he gets it." For my own part I felt that had he been a soldier instead of a professor he would have left his mark on the history of his country. As a teacher, I have never met his superior, and in teaching I have constantly tried to copy him. As to his personality, two separate estimates were formed. Those who did not know him, did

not like him. They did not understand his attitude of caution. They could not get him out of his shell. They were conscious that they did not know what he was at, or what he would do next. Those, however, who were his intimates and who knew him well, found him friendly, warm-hearted and genial, and as an intellectual companion, in a quite rare degree, interesting and entertaining. Yet he was always Humphry, and at his best when he was discussing some question in Surgery or Pathology with someone who he thought might throw some light upon it, but in whose harness he was determined, if possible, to find a weak spot. You will not, I think, Mr Vice-Chancellor, be surprised that I have availed myself of this opportunity of alluding to him, as one whose memory will long be green amongst us. To follow such a man, who grounded his claims to be remembered not only on the character of his work, but on

the long period during which he was a conspicuous figure amongst you, is so difficult an undertaking that any success that is possible to me can, I fear, be only very limited. But it shall be as far from failure as my best efforts can remove it.

As I have but recently come among you, as the representative of a not unimportant department in the field alike of science and of art, for, in both, Surgery now holds an established place, you may expect me, speaking to an audience which includes a considerable lay element, to say something of surgery not as surgeons study it, but as a subject, in which the public must feel a keen, and increasing interest, seeing that every advance which is achieved is an event which, in the case of illness or accident—which, however, may Heaven avert—might, by anyone amongst us, be found a valuable personal asset. I will venture to say that the changes which have

taken place in surgery in recent years are as great as those which have revolutionised so many other departments of human energy. Views which were, but lately, universally held have been completely and for all time swept away, and discoveries have been made which have fundamentally altered our conceptions, and enabled us to establish new principles, and to elaborate new methods of procedure. These results have been mainly due to the work of Pasteur and Lister,—work which must last as long as the discovery of the circulation of the blood by Harvey, or of the principle of the steam-engine by Watt. Before their time the surgeon was met at every turn by blood-poisoning, in the form of erysipelas, or some of its allies, and these complications were so certain to attack even small wounds that there were many operations which, warned by bitter experience, no one ventured to perform. And there were many

parts of the body, for instance the organs within the abdomen, which it was believed were so intolerant of interference, that any operation on them would be attended by almost necessarily fatal results. Thus while many operations were never attempted, even minor proceedings were frequently followed by the death of the patient.

The new starting point consisted in the discovery by Pasteur that many diseases in the vegetable and animal kingdoms were due to the action of minute organisms or *Bacteria.* The next step was the application of Pasteur's discovery to surgery by Lister, who commenced investigations into the use of substances by which these harmful micro-organisms, these bacteria, might be excluded or destroyed. I have heard Lord Lister describe his first experiences. He found that when he used carbolic acid in a case in which he had opened a large abscess, the

behaviour of the wound, and the subsequent progress of the patient, were absolutely different from what had hitherto been observed. He found also that wounds which before had taken six weeks to heal now healed soundly within a week,—or as the technical expression is, by first intention, and that whereas, previously, inflammation had occurred and a large discharge of matter had taken place, attended with fever, or with some dangerous form of erysipelas, or even mortification, now there was no inflammation, no matter was formed, no fever was developed, and recovery was rapid and entirely without complication. Can we not all understand Lister's astonishment, and the delight with which these results were contemplated?

Of course he was, at first, regarded as a visionary: some even thought him scarcely honest, so great was the difficulty of believing what he declared his experience to have been.

Surgeons who pinned their faith to established authority, and whose minds had passed the plastic stage, would have none of it. Men, however, who were younger and whose minds were receptive, and untrammelled by stereotyped impressions, hailed Lister's announcement with enthusiasm, and at once set to work to enlarge his observations, and improve upon his first attempts. But his glory must always be that he set this great stone rolling. He demonstrated the main fact as to the part played, in surgical cases, by micro-organisms in the production of disease. Following his lead, his disciples soon spread themselves over the whole field of surgery and constantly established new records of success. In short, if I may paraphrase Goldsmith's two well-known lines, I would say

> And still they worked, and still the wonder grew,
> Lister was right, and everything was new.

The thirty years that have since elapsed

have been years of revelation and advance, in every direction. While the fundamental principle remains the same, methods of procedure have undergone rapid development. Many agents have been ascertained to be efficient in dealing with these micro-organisms. First, there is cleanliness, secured by soap and water, often a very necessary first step, while carbolic acid, and mercury, in dilute solutions, are the substances most commonly, but by no means exclusively, employed in their destruction. These are used for the patient's skin, and for the hands of the surgeon and his assistants, and the nurses, while all instruments are prepared by so simple a method as boiling. Thus the patient, the surgeon and his instruments, are all protected, and the operation is rendered, as the term is, aseptic. The general results have far transcended the most sanguine expectations. It has been gradually disclosed to us, that there is no

organ anywhere in the body, which is not amenable to operation, no part which is so constituted, or endowed, that it cannot, under the aseptic method, be treated by surgical interference. Whereas, in the olden times, no surgeon dreamt of operating on the abdominal organs, every one of them is now operated upon with a degree of success which steadily increases, as experience accumulates, and methods of procedure are perfected. Whereas, to operate on a nerve was supposed to produce lockjaw, nerves now, if they have been divided, and the parts they supply are paralysed, can be rejoined as a sailor would join a rope. If a part of a principal nerve has been destroyed, the lost piece can be replaced by the spinal cord of a rabbit, with the result that complete recovery may take place. Perhaps, the most astonishing thing of all is, that there have been several cases, in which patients have recovered from

wounds of the heart, which have been sewn up.

The study of anatomy has now reached so advanced a stage, that those who are to be operating surgeons are able to prepare for their future work by making themselves intimately acquainted, not only with the structures of the body, and with minute particulars concerning their component parts, in their usual formation, and relations, but with the deviations from the normal, which when complicated operations have to be performed, might, if unknown, lead to failure, or serious difficulty.

The safety with which surgical operations can now be performed has added greatly to our resources in another direction. There are numerous cases, the exact nature of which it is impossible to ascertain by mere external examination. To make sure of the precise condition, nothing less than direct investiga-

tion will suffice. As matters at present stand, the required information can be obtained by what is called an exploratory operation, during which the part concerned can be fully examined, so that the surgeon may know, not only what can be done, but the exact manner in which it should be carried out.

A very important result of this development of surgery has been to produce a kind of secondary revolution, in the practice of medicine: for the only remedy in numerous cases which are termed medical consists in mechanical interference, in other words, in a surgical operation. In many forms of that grave condition, peritonitis, in many forms of malignant disease of the stomach and intestines, in gall-stones, and stones of the kidney, in some diseases of the brain, in paralysis depending on growths which press upon the spinal cord, in perforation of ulcers of the small intestine in the course of enteric fever,

and in numerous other instances, the surgeon can render the physician indispensable assistance.

Again, it has been found, that many of the most dangerous, or fatal diseases, met with in surgery, are in their simpler forms comparatively mild and easily treated. It is only when they are complicated by the action of bacteria, that is, when infection occurs, that they assume a grave character. Thus there are forms of mortification which, if the part attacked can be kept free from the influence of bacteria, in other words, if the aseptic condition can be maintained, remain confined almost entirely to the part originally involved, and they do not disturb the general health. But should bacteria be allowed to enter upon the scene, the whole aspect of the case is forthwith changed. The process of mortification becomes active, and rapidly extends, the "life of all the blood is touched

corruptibly," fever is developed, and frequently, in a few days, a fatal result is reached.

Furthermore, the revealing light which was thrown on the field of pathology, by the discovery of Pasteur, has enabled observers to discern the fact, that many dread diseases, for instance, tetanus, or lockjaw, are due entirely to infection by some particular micro-organism. Let such infection be prevented, and these diseases disappear from among men.

A very remarkable event in the chain of progress was the discovery of anæsthetics, or of those agents by which consciousness can be completely and safely suspended while operations are being performed. Here was the realisation of a dream which poets and philosophers had dreamt for centuries.

It took place in 1846, and was the work of an American dentist of the name of Morton. The agent he employed was ether. Shortly

afterwards, Simpson of Edinburgh discovered chloroform. Since those early days the subject has, of course, been carefully investigated, and at the present time several agents are regularly employed, either alone or in combination, in such a way that the peculiarities and susceptibilities of different patients may be met with the least amount of danger and subsequent discomfort. These agents are so much in demand, for example at St Bartholomew's Hospital—whence I have lately emigrated—that no less than four Anæsthetists are occupied exclusively in giving them. Last year as many as 8000 administrations took place in that Institution alone.

Under the system which has now been established, and in skilful hands, the administrations of these various agents is so free from risk that accidents scarcely ever occur. And to provide as far as possible for the

safety of the public, every student at the Hospital attends compulsory lectures and classes on the subject, and himself, of course under the most careful supervision, gives anæsthetics to from thirty to eighty cases. Similar arrangements are in vogue at all the other hospitals.

At the present time every surgical operation which entails more than slight and momentary suffering, whether it be the removal of a firm tooth or one of the most formidable and critical operations, anæsthetics are given as a matter of course, as they are also in the numerous examinations that are called for, especially in those in which it is necessary that the muscles should all be relaxed.

Surely this discovery of anæsthetics was one of the most priceless gifts that have ever been conferred on the human race, particularly as it was made before the dawn of

the new surgery, for without it, obviously, surgery in its recent developments would have remained an impossibility.

And now, Mr Vice-Chancellor, I will not detain you longer, but while heartily thanking you for having spared time to pay me the honour of your presence to-day, and for many acts of courtesy and kindness which I have already received at your hands since I assumed my office under your jurisdiction, I will conclude by quoting words which were recently used by the Master of Trinity—and which appear to me to embody a grand and exalted conception, and to be instinct with noble eloquence:

"Let us all," said he, "if we can, agree in this, that the noblest function of this ancient Mother of us all is to be a source of light; to cherish and diffuse what has been revealed already; to believe and hope that there is always more on its way; to search the heavens for it before it has shone; to hail it

with joy when at last it dawns; to diffuse it also, without reserve, to all; and finally, to own with humblest reverence at each glorious discovery, 'Not unto us, O Lord, not unto us, but to Thy Name give the praise.'"

Cambridge: Printed at the University Press.

11

GENETIC INTERPRETATIONS IN THE DOMAIN OF ANATOMY

Presidential Address before the Association of American Anatomists

BY

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GENETIC INTERPRETATIONS IN THE DOMAIN OF ANATOMY.*

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The science of anatomy, although one of the oldest of all sciences, was long neglected in America, and taught only in a routine fashion by professors who had little or no thought for the promotion of the science or any aim higher than teaching a certain number of established facts in gross anatomy to the maximum possible number of students. Within the last generation the few pioneers of anatomy have been succeeded by teachers, many of whom share the highest ideals of anatomical science, and have contributed important discoveries by which it has been really advanced. Our Society is at once the symbol and the outcome of these comparatively new conditions in America, and we have as our duty not only actively to encourage research, to spread anatomical knowledge, and to earn appreciation of anatomy as a living science, but also to exert a missionary influence by which the dignity and vitality of our science shall be brought to recognition at all our universities.

* The following recent or new technical terms are used in the course of the address and are recommended for adoption.

Cytogenic glands, false glands which produce cells, as for example, the lymph and genital glands.

Cytomorphosis, to designate comprehensively all the structural modifications which cells or successive generations of cells may undergo from the earliest undifferentiated stage to their final destruction.

False glands, all glands, which develop without ducts.

Lymphæum, a more or less definitely circumscribed area consisting of cellular reticulum, the meshes of which are charged with leucocytes and are in direct communication with lymph-vessels or more rarely with blood-vessels. It is a site for the multiplication of leucocytes.

Mesepatium, the membrane (French, *méso*) extending from the stomach and duodenum to the median line of the ventral abdominal wall, and in which the liver develops. It comprises a *dorsal* mesepatium (lesser omentum) and *ventral* mesepatium (falciform or suspensory ligament).

It is sometimes said, and perhaps more often thought, that anatomy is a completed science. This assertion is based upon the thoroughness and exhaustive character of the descriptions to be found in our text-books of the anatomical conditions in the human adult; yet even as regards the organization of the adult we have still much to learn, especially concerning the microscopic structure with which we are still very imperfectly acquainted.

But anatomy is not alone a descriptive science. It is also comparative and genetic. In both these directions its development is very far from complete, and a vast amount of original research must still be completed before comparative anatomy and embryology shall have approached anywhere near even the present perfection of descriptive human anatomy.

To embryological research must be attributed a large part of the extraordinary progress which anatomy has made during the last twenty-five years. By embryology we have gained a far deeper understanding of all anatomical forms, we have acquired new interpretations for pathological facts, and we have secured for the first time some clear insight into the essential structure of the brain. I need not do more than allude to these achievements, since they are familiar to us all, and have most profoundly affected our anatomical conceptions. Our point of view has changed, and we interpret the anatomy of the adult in terms of the genesis of the organs and tissues during their embryonic development.

Perhaps no man has contributed so much towards this result as the great Leipzig anatomist, Wilhelm His, whose death this year we have to lament. He was a great master. He had full command over the problems of anatomy and contributed in the richest measure to their solution. His influence in America has been especially strong and widespread, and has certainly had much to do in bringing about the progress of anatomy in this country, which we are seeking to maintain, and if possible, increase. In what I have to say to you on this occasion, you will perceive

Phrenic area, the area on the superior or cephalad surface of the liver, by which the liver is attached permanently to the diaphragm. It includes the whole of the territory of the coronary and triangular "ligaments," so-called in current text-books.

Sinusoïd, an irregular blood space, produced by the subdivision of a larger blood-vessel by the ingrowth of the parenchyma of an adjacent organ.

Structural unit, the territory of an organ supplied by a single terminal branch of an afferent vessel (artery or vein); the volume of such a unit is often only 10-20 cubic millimeters.

Trophoderm, the special layer of cells formed on the exterior of the young mammalian blastocyst, and serving to secure the implantation of the ovum in the uterus.

the influence of His clearly, and I cannot let the opportunity pass of expressing publicly my gratitude and admiration for the greatest anatomist of his time.

Although embryology has already contributed in so ample measure to the promotion of our science, we are still far from having accepted all the enlightenment which she offers us. With your permission I will try to present to you certain embryological aspects of anatomy, the character of which I have sought to indicate in the title of this address, by the words "genetic interpretations."

First of all, let us consider the subject of cytomorphosis. This word I proposed in 1901[1] "to designate comprehensively all the structural modifications which cells or successive generations of cells may undergo from the earliest undifferentiated stage to their final destruction." As stated on that occasion it is convenient, though somewhat arbitrary, to distinguish four fundamental successive stages of cytomorphosis. These stages are (1) the undifferentiated stage: (2) the stage of progressive differentiation, which itself often comprises many successive stages; (3) the regressive stage or that during which degeneration or necrobiosis occurs; and (4) the stage of the removal of the dead material. The general data on which the conception of cytomorphosis is based have been briefly put together also in my text-book of embryology, and it seems therefore superfluous to dwell upon them at length in addressing you.

I cannot of course claim any greater originality in the establishment of the conception of cytomorphosis than is implied by the definite formulation of the ideas upon which it is based. These ideas have been gradually gathered as the fruit of numerous investigations in histogenesis. The mentioned investigations have made us all familiar with the conception of undifferentiated embryonic cells, with the gradual progress of differentiation in the cells during the embryonic, fœtal, and even post-natal periods; and have also made us acquainted with various examples of degeneration and atrophy occurring in the course of development, both before and after birth. Up to the time when I proposed the term there had been, so far as I know, no attempt to survey all this array of facts from a single unifying point of view. But such a point of view is, I believe, well calculated to render our notions more precise as to many processes of development, and to afford us at the same time the practical benefit of being able to present the facts of histogenesis in our teaching in a way, which is very advantageous, because it facilitates in the student's mind the establishment of a real insight into the general course of development by emphasizing principles of very wide application. To me, at least, it seems that the conception of cytomorphosis should be made the

foundation of all our instruction in anatomy, and that its importance should be constantly emphasized in our class-rooms and that when good illustrations of cytomorphosis are encountered by the student, his attention should be especially directed to them, so that he may become familiar with the conception. Let me mention a few illustrations which I have found serviceable in teaching.

But first I must call your attention to an aspect of cytomorphosis, which has not hitherto, so far as my knowledge goes, been sufficiently emphasized. We may distinguish two fundamental phases. During the first, cell division occurs, during the second, cell division does not occur. During the first phase we may find a progressive alteration, which gradually takes place in successive generations of cells, but apparently the amount of differentiation which can occur while cells retain the power of active division is comparatively slight. During the second phase, since the cell no longer divides, the alteration takes place in the single cell, and the alteration, which occurs under these conditions, is typically great and may be best designated by the term final differentiation, differentiation being here held in our minds clearly distinct from degeneration. By final differentiation we mean the establishment of that special organization of a cell, which brings to perfection the specialized physiological function for which the cell is destined. Thus the alteration of a mesenchymal cell into a muscle fibre is its final differentiation, and establishes the physiological perfection of that cell as a contractile element. Beyond the final differentiation of the cell comes the series of degenerative changes. A comprehensive study of cell degeneration is yet to be made, nevertheless we can already say that, although cell degeneration is chiefly characteristic of the second phase of cytomorphosis, which is also characterized by the cessation of cell division, yet the degeneration may be initiated before the power of cell division is lost and the degenerative change in the cells may go on while they are still proliferating; but typically it seems rather that degeneration belongs to the second phase of cytomorphosis, and this seems to be alike true for necrosis and atrophy, that is to say, simple cell death, and for necrobiosis, that is to say, cell death preceded by structural changes, which we know commonly under the name of hypertrophic degeneration.

Let us pass on now to a few illustrations of cytomorphosis:

The first to which I would direct your attention is afforded by the formation of the "*trophoderm.*" This is a new term which I have recently brought forward to designate the special layer of cells formed apparently from the ectoderm (or according to Assheton's theory, from

the entoderm) which serves to secure the implantation of the mammalian ovum in the walls of the uterus. In my "Text-Book of Embryology" I have figured these cells from the human ovum and applied to them the term trophoblast, but as Professor Hubrecht, who introduced this last term into science, has objected to this application of it, it has been necessary to introduce a new term, hence the designation trophoderm. It corresponds in large part, perhaps wholly, to that which Duval designated as the ectoplacenta. It is the first tissue in the mammals to be distinctly differentiated. The cells by their large size, distinct boundaries, and characteristic nuclei, are readily distinguished from any other cells existing in the embryo at the time the trophoderm is differentiated. Very soon after the development of the trophodermic cells, a large part of them begin to complete their cytomorphosis by undergoing degeneration and resorption. By their disappearance, as I have elsewhere pointed out, the intervillous spaces arise. The trophoderm therefore is not only the earliest tissue to be specialized in the development of mammals, but also the earliest tissue to absolutely complete its cytomorphosis.

Another striking illustration of the cytomorphic cycle with its phases of differentiation, degeneration, and disappearance of cells is offered to us by the blood corpuscles. The first blood corpuscles are cells with a minimum amount of protoplasm. The cells then proceed to grow, and as they grow, differentiate themselves in part at least, into red blood globules. In mammals there follows the stage, degenerative in character, by which the nucleus of these red blood corpuscles disappears. The manner of its disappearance is, to be sure, still perhaps a matter of debate, but for us for the moment is of minor importance. After the degeneration or disappearance of the nucleus, the blood corpuscles are destroyed and, having completed their cytomorphosis, are replaced by fresh ones.

A third admirable illustration is offered us by cartilage, and a fourth by bone. In cartilage we see at first a differentiation of simple mesenchymal cells which then enlarge, becoming the characteristic cartilage cells. When ossification of the cartilage occurs we can easily follow the hypertrophic degeneration of these same cartilage cells, which has been so much studied that good accounts of the enlargement and breaking down of these cells preliminary to the ingrowth of the osteogenetic tissue can be found in all the better text-books of histology; but I regret to say I do not recall any text-book either of anatomy, histology, or embryology, which points out the fact that this succession of changes in cartilage cells is a typical and almost perfect illustration of cytomorphosis. Almost the same can be said of bone, for in the formation of this tissue also we have first, the differentiation of the mesenchymal cells into osteoblasts, which

are always of larger dimensions than the cells from which they arise; and after these osteoblasts have become bone cells they cease their development and apparently degenerate. I have to say *apparently,* because, so far as I know, the fate of the bone corpuscle has not been ascertained with certainty. We risk but little, however, in asserting that the bone cells also offer an instance of a normal, complete cytomorphosis.

As a fifth and last illustration, let us choose the epidermis, in which we have a distinct type of differentiation. In the basal layer are the cells, which divide and produce, according to our present notions, all of the cells of the epidermis. When the basal layer cells divide, however, some of them only, pass immediately through further cytomorphic changes in order to make first, the cells of the mucous layer, and later, by undergoing cornification, to constitute the horny layer. Others of the basal cells remain members of the basal layer and continue to proliferate. We thus see the progeny of the original basal cells divided into two classes: the cells of one class pass on in their development, the others retain their ancestral type. In the epidermal cells we observe as in other instances of cytomorphosis, first the enlargement and differentiation of the cells, here occurring in the mucous layer, and later their degeneration or cornification followed by their necrosis and destruction.

It would be easy to multiply these illustrations. All of you could supply more. That which I would urge upon your consideration is the value of the cytomorphic interpretation in explaining the origin and differentiation of tissues in the light of the broadest principle of cellular development which we have up to the present time been able to establish.

I will now ask you to consider certain possible genetic classifications. The most fundamental and important of these seems to me to be that of tissues and of organs according to the germ layers from which they arise. This classification was made the basis of his entire course of lectures upon animal morphology by Professor Carl Semper, the Würzburg zoologist, under whom I had the pleasure of studying in 1875-76. It is not merely very practical and advantageous alike to teacher and pupil, but is also the only thoroughly scientific classification of structures and organs which we can adopt. No other classification should, in my judgment, be seriously considered. So firmly do I hold this conviction that I greatly deplore the fact that our text-books of histology are not written upon an embryological basis, the lack of which deprives them of much of the scientific character and value they ought to have.

As our knowledge of the development from the germ layers has grown, we have learned with ever-increasing certainty that each germ layer has its specific rôle to play. Each germ layer produces its own specific

set of tissues, which are not duplicated by the tissues of any other germ layer. I have already pointed out on another occasion that the importance of the germ layers is as absolute and unvarying in the domain of pathology as in normal differentiation; I need not dwell on that aspect of the question now, but will only repeat the declaration of my belief that the entire teaching of the pathologist as well as of the histologist and anatomist should be based on the doctrine of the germ layers and their specific rôles in histogenesis.

Almost any group of tissues would offer a favorable opportunity for the discussion of genetic classification. We may select those which are differentiated from the embryonic mesenchyma and which are commonly grouped in the adult under the names of the connective and supporting tissues. It is almost superfluous, so much is the genetic point of view neglected, to call your attention to the fact that in our current text-books of histology there is often little or nothing which would enable the student to grasp the relations of these tissues to one another or to understand their genetic relationships. It is true that our knowledge in spite of the great advances of recent years is still too incomplete to justify our asserting that the classification which we can now make is final. Nevertheless we can already perhaps attain approximate finality. A very great step in advance was made when the character of the cellular reticulum was established and it was shown that this tissue is different from ordinary connective tissue. It has two principal characteristics: first, the matrix or intercellular substance is nearly or absolutely fluid, so that leucocytes can wander freely in the intercellular spaces of the reticulum; second, the network of original protoplasmic filaments has become directly converted into a network preserving more or less the original form, but consisting not of protoplasm, but of a new chemical substance, reticulin. Where cellular reticulum is developed, as for instance in the so-called adenoid tissue, there may be formed from the cells a minimum amount of connective tissue fibrils and of elastic substance, but if we may judge from our present knowledge the cells, which have produced reticulin, preserve but a very small capacity for the production of other elements of connective tissue. Hence, it seems to me that we may well put cellular reticulum in a class by itself, quite apart from the true connective tissues in which the intercellular substance is not mainly fluid and in which there is an abundant development of fibrillar or elastic substance, or of both, and in which, further, reticulin is nearly if not wholly absent. We shall thus come to place all the connective tissues, properly so-called, in a second genetic group. When we follow in the embryo the history of young connective tissue, we learn that it undergoes two principal kinds of modifica-

tions, those affecting the matrix, and those affecting the cells. On these differences the classification in the following table is based.

We also know that connective tissue can be directly transformed into cartilage, which, therefore, unquestionably belongs in the same second genetic group as the true connective tissues. As regards bone, I find it somewhat difficult to reach a decision, but incline to the conclusion that bone should be regarded as distinct from the true connective tissue, thus making a third genetic division of the tissues derived from the primitive mesenchyma. This conclusion appeals to me partly as a protest against the absurd, though long established and honored, custom of separating cartilage from connective tissue, and putting cartilage and bone together in a common group under the head of supporting tissues. The following table presents the proposed classification in a form which you can easily follow:

TABLE I.—THE MESENCHYMAL TISSUES.

MESENCHYMA	Cellular reticulum		
	Embryonic connective tissue	Matrix specialized	Mucous tissue
			Adult connective tissue
			Cartilage
		Cells specialized	Fat cells
			Pigment cells
			Smooth muscles
			Basement membranes
			Pseudo-endothelium
			Genital interstitial cells, etc.
	Bone		

Let me refer briefly to a third and more special example of the genetic classification of tissues, namely, that of the blood-vessels. As you probably all know, recent embryological investigations have compelled us to recognize not only the three familiar and long-known classes of blood-vessels, arteries, veins, and capillaries, but also a fourth class, that of the sinusoids. Capillaries arise as small vascular sprouts from pre-existing vessels, and these sprouts grow in the mesenchyma. A sinusoid, on the contrary, has an entirely different developmental history, for it is produced by the subdivision of a pre-existing and relatively large vessel. The subdivision is accomplished by the proliferating tubules (or trabeculæ) of an organ, which encounter a large vessel and invade its lumen, pushing the endothelium before them.[4] The endothelium of the vessel, on the other hand, expands and spreads over the tubules (or trabeculæ). By the convolutions and anastomoses thus produced, a large vessel is subdivided into small ones. It follows that a sinusoidal circulation is purely venous or purely arterial. It may suffice, upon this occasion, to point out again that the structure of many important organs, as for

example, the liver and Wolffian body, cannot be understood or even described correctly without taking into consideration the sinusoidal character of their circulation. In this case also, the adoption of the genetic interpretation is much needed. We shall apply presently the concept "sinusoid" to aid us in the interpretation of glands.

We may pass now from a consideration of tissues to that of organs, and begin with the glands. The classification, which at the present time prevails widely, is one based upon certain incidental peculiarities in the shape of the secretory portion of the glands, so that they are put into two main divisions: the tubular and alveolar; then under each of these we have three main parallel groups:

the simple tubular or alveolar glands;
the simple branched tubular or alveolar glands;
the compound branched tubular or alveolar glands.

But in this classification there is no place for such a gland as the liver and the thyroid. In text-books of histology, we find the liver tucked in under the tubular glands and designated as a reticulated tubular gland, and the thyroid placed as a follicular gland under the head of the alveolar. But in another way the system also fails, for there are tubulo-alveolar glands which again must be classified as simple, simple branched, or compound branched. Essentially this classification is adopted by the authors of the manuals of histology which I have examined. To classify glands thus, seems to me about on a par with classifying organs by their being solid or hollow, a principle, which would put the spinal cord in the same class with the intestine, and nerves in the same class with the tendons. The peculiarities of shape of the secretary portions of glands are entirely secondary, and do not indicate anything fundamental in regard to the structure of the gland itself. We cannot call a system good which, if applied in accordance with its own definitions, would put some of the mucous glands of the stomach in one division, others in another division, because, although these glands are alike in their histological structure, some of them are branched and some are not. Must we not condemn a view, which excludes the ovary from the glands and makes the testis a compound tubular gland, although ovary and testis are strictly homologous organs, even in the details of their structure? These are only samples of the innumerable difficulties which the system encounters, because it is essentially pedantic, admirable as an orderly arrangement of names, but impossible as a presentation of anatomical facts.

It appears to me not difficult to make an entirely new classification of

glands, which shall be based upon their genesis and upon the morphological distinctions, which exist between them. To begin with, we may put the unicellular glands, of which the goblet-cells serve as the most familiar type known in man, in the Class A, v. p. 256; next we may have the true multicellular glands of the epithelial type, which always develop with ducts by which their secretion is discharged; these form Class B; while a third class would include the false glands which never develop with ducts, which produce either merely an internal secretion so-called, or are adapted to the development of cells of special kinds, as, for example, the lymph- and genital-glands; such structures constitute Class C.

We must first attempt a classification more in detail of the true epithelial glands (Class B). In my opinion we can best make two fundamental divisions. The glands of the first division have often been called single or simple follicular glands; I propose for them the term "*simple glands.*" The glands in question are always small and have one or several centers of growth according as they are simple tubes or slightly branching. Those of each kind are always very numerous and they occur more or less near together over considerable areas. There are two types of these known. The first are simple invaginated areas with scattered unicellular glands, as for instance the glands of the large intestine, the so-called Lieberkühn's follicles; they might be called simple follicles. Glands of the second type are invaginated areas with specialization of the cells, as, for example, the sweat, gastric, and sebaceous glands; they might be called glandular follicles. In the accompanying table the principal glands of this division are enumerated.

The glands of the second division are of greater bulk and are often referred to as organic or branching glands. I propose to name them "*compound glands.*" They are provided with a single main duct, which is more or less freely branched, each branch connecting finally with the secretory portion proper of the organ, which portion may itself also be branched or not. Each gland falling in this division is a more or less complete organ by itself, receiving its special blood supply, and its special innervation—is, in short, a clearly marked physiological entity. Such a gland differs profoundly in its plan of organization from the glands of our first division. Of the second division there are clearly three main types to be distinguished. In the first type the branches of the glands are found to be supported by mesenchyma or its derivative, connective tissue, which is more or less abundant between the ducts and secretory elements of the organ, and in the mesenchyma there is a capillary circulation, which is often brought, however, into intimate proximity with the epithelial elements of the organ. These organs are further character-

ized by the fact that their branches remain distinct. In the second type, on the other hand, the branches unite together and form an anastomosing gland structure, and when this anastomosing condition is found it is associated, not with the development of connective tissue and capillaries, between the epithelial elements of the organ, but, on the contrary, with the presence of a sinusoidal circulation. The branching glands with capillary circulation are numerous, and they may arise, as is noted in the table, from either the ectoderm, the entoderm, or the mesothelium. Glands of the second type, anastomosing and furnished with sinusoids, are few in number. The liver is, of course, the most typical and the most important. With the liver we ought perhaps to associate the paraphysis, for the recent and still unpublished investigations of Dr. John Warren show that when this gland is highly developed it is of an anastomosing type, and make it probable that its blood supply is sinusoidal.

There remains still a third type, which is necessary, because the ducts become obliterated in a certain number of true epithelial glands, which develop primarily with ducts. There results in each case a group of hollow epithelial follicles, which are characteristic. For this type I propose the name, "*ductless epithelial glands.*" The thyroid gland and the hypophysis are probably the best-known illustrations of this group of glands. Although the morphology of the pineal gland (epiphysis) is obscure, the organ seems at present to belong to our third type.

Our third Class, C, comprises the false glands, which never develop with ducts. So far as I am aware this statement may be made absolute for all glands of this class. It is true beyond any possible question for most of the glands, which are here to be considered, but it is perhaps as well to note that possibly some of the glands of the first division may be found in some vertebrates to have been primitively provided with ducts. This seems to me possible, but not probable. The first division of the false glands are the epithelioid. They are perhaps exclusively, so far as the essential gland elements are concerned, of entodermal origin, and it has become probable that their circulation is typically sinusoidal. In the present state of our knowledge it would be venturesome to make positive assertions on these two points. In the epithelioid glands we have groups of cells of epithelial origin separated, in the adult at least, from the layer which produced them, and brought into intimate relation with blood-vessels. A second division comprises the mesenchymal ductless glands, which are similar to the epithelioid glands in their general appearance, but their specific elements are derived from the middle germ layer. As an illustration of the ductless false gland of the first division, I may mention the parathyroid; of the second division, the suprarenal cortex. As

to the position of the thymus, I feel quite uncertain and hardly dare to say whether it should be placed among the epithelioid glands or among the cell-producing glands. Similarly, how to place the interstitial cells of the genital glands in our system is not yet quite clear to me. The third division is that of the cytogenic glands, and of these we may readily distinguish three important types: the first, those in which lymph cells arise; second, those which produce red blood corpuscles; and third, those which yield the genital elements. The glands of the first type may be called lymphæal structures. "Lymphæal" is a new term derived from "Lymphæum," itself a new technical expression, which I have used for several years in my lectures on histology and have found advantageous. A lymphæum may be defined as follows: it is a site for the multiplication of leucocytes and is a more or less definitely circumscribed area consisting of cellular reticulum, the meshes of which are charged with leucocytes and are in direct communication with lymph-vessels, or more rarely with blood-vessels. The following offer examples of lymphæa: solitary follicles, tonsils, thymus, lymph glands, hæmolymph glands and spleen. As stated above, whether the thymus should belong in the first or third division, I cannot say. Of the second type in this division, the bone marrow is the most important example. Of the third type, that of the genital glands, we have of course to distinguish two forms, the ovary and the testis.

With these explanations, I hope the accompanying table will be clear and I trust that the proposed new classification of glands will seem to you both more scientific and more available than the classification now prevalent, which I should like to see displaced.

TABLE II. CLASSIFICATION OF GLANDS.[1]

CLASS A. *Unicellular.*

CLASS B. *True Glands*, always developed with ducts.

DIVISION 1. *Simple Glands*, (*unifollicular or single glands*).

a. Ectodermal.
   1. Sweat glands.
   2. Sebaceous glands.
   3. Buccal glands.

b. Entodermal.
   1. Œsophageal.
   2. Gastric.
   3. Intestinal.

c. Mesothelial.
   1. Uterine.

DIVISION 2. *Compound Glands (organic or true compound glands).*

Type a, ductless epithelial branching (*with capillary circulation*).

1. Ectodermal.
   Salivaries, tear gland, Harderian.
   Mammary glands.
2. Entodermal.
   Pancreas.
3. Mesothelial.
   Appendicular glands of the urogenital system.

Type b, anastomosing (*with sinusoidal circulation*).

1. Liver.
2. Paraphysis (in Necturus).

Type c, ductless epithelial (*with secondary obliteration of duct*).

1. Thyroid.
2. Hypophysal gland.
3. Infundibular gland.
4. Pineal (*epiphysis*).

CLASS C. *False Glands*, never developed with ducts.

DIVISION 1. *Epithelioid glands (exclusively entodermal?)*

1. Parathyroid.
2. Carotid.
3. Thymus (*cf.* below) (?).

DIVISION 2. *Mesenchymal ductless glands.*

1. Suprarenal cortex.
2. Coccygeal gland and other chromaffinic cell organs.
3. Interstitial cells of genital glands (?).

DIVISION 3. *Cytogenic glands.*

a. Lymphæal structures.
   1. Lymph glands and follicles (tonsil?).
   2. Hæmolymph glands.
   3. Spleen.
   4. Thymus (?).

b. Sanguifactive organs.
   1. Bone marrow.

c. Genital glands.
   1. Ovary.
   2. Testis.

I should like to include, in passing, reference to another general anatomical conception which, though not based strictly on embryological results, may be appropriately mentioned. I mean that unit of adult organization, which is sometimes referred to as the "*lobule*," but, as this term is somewhat confusing owing to the manifold meanings assigned to it, I venture to express the hope that the term "*structural unit*" will be

used instead, as has already been done by a few writers. We can then continue to employ the term lobule for the lung and the liver in the senses tradition gives to the term, as used for these two organs, and avoid confusion. The *structural unit*[8] may be defined as the territory of an organ supplied by a single terminal branch of an afferent vessel (artery or vein). The volume of such a unit is often only 10-20 cubic millimeters. In the case of the liver, the structural unit comprises parts of several adjacent so-called lobules. It is a pleasure to recall that the recognition of the anatomical importance of these units is due to one of our most distinguished American investigators, Dr. Mall.

Finally, I should like to apply the principle of genetic interpretation to descriptive anatomy. It will, I think, sufficiently expound the point of view I am advocating to consider the application of the principle to a single organ, and for this purpose we may conveniently select the liver. In order to show that what I propose is practically a real and great innovation, let me indicate to you briefly the character of the anatomical descriptions of the liver to be found in some of the leading text-books of human anatomy.

In Cunningham's Anatomy (1902), the account of the liver is written by Professor Birmingham. He describes, 1, the general form of the surface; 2, the topographical relations and surfaces in detail; 3, the fissures, without giving their morphological relations; 4, the division into right and left chief lobes; 5, the peritoneal relations and ligaments; 6, the physical characteristics.

In the tenth edition of Quain's Anatomy (1896), the description opens, 1, with the dimensions and weight; 2, the surfaces; 3, the fissures; 4, the ligaments and the omentum; 5, the topographical relations; 6, vessels and nerves; 7, the ducts.

Testut in the third volume of his Anatomy (1894) gives, 1, the situation; 2, fixation; 3, volume and weight; 4, general confirmation, including the two chief right and left lobes; 5, the surfaces in detail.

The account of the liver in Poirier's Anatomy, Volume IV (1895), is written by Charpy, who begins with 1, the definition, and continues with 2, situation; 3, fixation; 4, data as to weight, consistency, etc.; 5, the form and surfaces. Under the head of fixation Charpy says:

"**La foie est suspendu à la voute du diaphragme par deux moyens d'attache: par des replis péritonéaux et par la veine cave inferieure.**"

This misleading statement is the more deplorable because he mentions only incidentally that the liver adheres directly to the diaphragm. Quite at the end, the division into the right and left lobes is mentioned.

In the fourth edition (1901) of Merkel-Henle's Grundriss, there is, 1, a general account, which is distinctly not morphological in character; 2, detailed description of the surfaces and topography; 3, of the histology.

Gegenbaur in the seventh edition of his Anatomy (1899) proceeds very differently, for he has strong morphological inclinations. He gives, 1, the general account of the development of the liver; 2, general account of the surfaces, including the division into the chief lobes; 3, the relation of the veins to the omentum and the falciform ligament. Gegenbaur is the only author of a text-book of human anatomy, known to me, who gives a distinctly morphological account of the human liver, but even his presentation of the subject leaves much to be desired, chiefly because his knowledge of embryology was meagre, and quite insufficient for an adequate interpretation.

It would be easy to analyze descriptions in other text-books, but enough has been presented to show that they are usually characterized by certain common tendencies. The authors dwell upon the position and shape of the liver, seeking to emphasize its exact form, but not endeavoring at all to emphasize the essential characteristics of the organ, or to bring out the significance of its parts in a manner satisfactory to either an embryologist, a physiologist, a morphologist, or a pathologist. With the exception of Gegenbaur, none of the accounts rises above the level of sheer description.[5] They simply perpetuate the tradition inherited from the time when human anatomy was only the description of what was actually found in the human adult. That tradition has undoubtedly been in part maintained by the demands of surgeons, whose interest is necessarily chiefly given to the exact determination of the topographical divisions in the body, hence the influence of the surgeons, when dominant in the anatomical laboratory, has often exerted an influence unfavorable to the becoming maintenance of a scientific spirit, such as we ought to insist upon for the sake alike of anatomy and medicine.

If we review collectively the brief analyses just given of the actual descriptions in the text-books, we realize at once that those points, which the genesis of the liver reveals to us as fundamental, are scarcely heeded by the authors whom we have reviewed. This is not a fitting occasion to attempt a new description of the liver, and I can merely indicate to you the principal points upon which a scientific description ought, in my opinion, to be based. No little study and care would be necessary to work out practically the suggestions, embodied in the following schedule. Indeed, the schedule can doubtless be improved by others.

In order to prepare an adequate description of the liver, we must begin by laying aside certain bad habits which we have inherited and have

allowed ourselves to perpetuate. I mean the habit of applying the term ligaments, and the habit of applying the term fissures, to the liver; also the habit of describing the hepatic segment of the vena cava inferior as a vessel distinct from the liver, it being in reality, strictly, in every sense of the word, a portion of the organ. It may be further suggested that the introduction of a new term, *mesepatium*, may assist in clarifying the relations. The "*mesepatium*" is the membrane (French *méso*), which stretches from the ventral border of the stomach and duodenum to the median line of the ventral abdominal wall. It is in this membrane that the liver develops. Above the liver, between it and the stomach, is the dorsal mesepatium (lesser omentum). Between the liver and the body wall is the ventral mesepatium (falciform or suspensory ligament). Instead of speaking of the ligaments, we should speak of the insertion of the dorsal and ventral mesepatium into the liver; and instead of coronary and triangular ligaments, we should speak of the attachment of the liver to the diaphragm. This area of attachment might be called, as regards the diaphragm, the hepatic area, as regards the liver, the phrenic area.

With these preliminary explanations in mind, it may be suggested that a description of the liver must begin, as many authors have begun it already, with a general statement in regard to the position, size, color, and general form of the organ, and explaining that it is a gland, with a duct opening into the duodenum, and having the gall bladder appended to it, and that the circulation is sinusoidal, and not capillary.

Next, I should place a careful statement of the fundamental relations, as follows, *first*, of the broad connection of the liver with the diaphragm. This connection is primitive embryologically, is maintained throughout life and constitutes the phrenic area. It is not by the so-called ligaments or peritoneal folds, nor is it by the vena cava inferior that the liver is attached to the diaphragm. On the contrary it is by a large and characteristically shaped phrenic area of the organ that the connection is established. *Second*, the relation of the liver to the mesepatium, pointing out especially that the insertions of the dorsal and ventral mesepatia mark the division of the liver into right and left lobes and that the insertion is enlarged at one point towards the right to form the so-called porta of the liver, which admits from the dorsal mesepatium the hepatic artery, bile duct, and portal vein. *Third*, the relation of the veins to the organ, emphasizing that the portal vein marks the border of the dorsal mesepatium, and that its branches within the organ mark the so-called portal canals; emphasizing also that the umbilical vein or venous ligament marks the free edge of the ventral mesepatium, and explains the position,

origin and adult state of the ductus venosus. *Fourth,* the entrance and exit of the vena cava inferior. In this connection there should be made clear the rôle of the caval mesentery in furnishing a path for the *cava inferior*, and at the same time shutting off the lesser peritoneal space, and keeping the surface of the Spigelian lobe as part of the boundary of this space.

Next, again, might be presented the secondary features, especially the marking off of the caudate lobe from the chief right lobe by the vena cava inferior, and the marking off, similarly, of the quadrate lobe by the porta and the gall bladder.

Finally, according to this schedule, the description of the finer surface modelling and the contact with various adjacent organs, such as the stomach, colon, duodenum and kidney. Not one of these topographical relations is indispensable for a comprehension of the general character of the organ. Even from the standpoint of the surgeon and physician they are of minor importance. If they are put, as has been customary, in the forefront of text-book descriptions, attention is distracted from more essential things. Surely one need not argue to prove that a general comprehension of each organ is, first and last, the most important goal, to be striven for in the study of it.

In regard to almost every organ in the body it may be said, I think without injustice, that the current anatomical text-books offer bare and barren form-descriptions, seldom giving much, and often giving no, consideration to the essential morphological features of the parts. Take, for example, the urogenital system. We all know that the internal female genitalia are formed of two urogenital ridges, which fuse in the median line, making the so-called genital cord. There is in each ridge a longitudinal epithelial duct, which becomes the Fallopian tube, and by fusing with its fellow in the genital cord, produces the cavity of the uterus and vagina. A projection on one side of each ridge forms the ovary. Where the ridges have not united, rudiments of the Wolffian body of the embryo occur. The surface of the ridges, both where they are separate and where they are united, is covered by mesothelium. Around the duct (Fallopian tube), there is developed a muscular layer, and around the uterine portion of the fused ducts in the female a very powerful musculature is developed. By the union of the two ridges a partition is formed across the pelvic end of the abdomen, so that the abdominal cavity forms a pocket on the dorsal, and another on the ventral, side of the genitalia. Now the anatomical way of describing these organs is not to mention the ridges at all, but in the case of the female to speak of the uterus and its liga-

ments. It seems sometimes as if a deliberate effort were made by the descriptive anatomist to exclude all liberal use of the understanding and of the intelligence from the study of anatomy, and to reduce it almost to mere memorization of shapes and proportions, exceedingly difficult to fix in the mind by that method.

Is one not justified in condemning with great severity the perpetuation of this old type of anatomy? Is it not a grave mistake to fail to take advantage of the progress of anatomical science, and to utilize the best results of anatomical investigation to aid us in forming for ourselves, and still more, perhaps, for our students, clear notions of the essential characteristics of human organization? There has been within the last twenty-five years a very great progress in our knowledge of the topographical anatomy of the viscera, both thoracic and abdominal. When I plead for the presentation of the subject from the genetic standpoint, I do not mean to imply that this superior topographical knowledge should be slighted, but, on the contrary, I believe that if the student can first master the essential morphological relations of the body, it will be easier for him to master subsequently the finer, and often practically important, topographical details. Let our motto be, not "to memorize," but "*to comprehend*" the facts of anatomy.

Embryology illuminates anatomy. Its teachings give us the intellectual mastery of anatomical science, because embryology analyzes details, discriminates the essential from the secondary facts, and establishes the genetic interpretation, in the solvent light of which the obscurities of ancient anatomy vanish, and we see, where before was a dead sea of innumerable facts, new vital laws arising and guiding principles.

NOTES.

1. P. 247. Cytomorphosis was first used in the Middleton Goldsmith Lecture for 1901, entitled "The Embryological Basis of Pathology," *Science*, XIII, 481.

2. P. 256. Perhaps all or some of the salivary glands are entodermal. The submaxillary gland belongs among the organs, when it is a single large compound gland with a Bartholini's duct. When the submaxillary is represented by a group of small glands, they belong with the other simple buccal glands.

The position of the mammary gland must remain uncertain, until we can decide whether it is merely a group of glands, or morphologically a true compound gland. The significance of its peculiar development is still unsettled.

The hypophysis will perhaps, with more accurate study, be found to be an anastomosing gland with a sinusoidal circulation.

3. P. 258. The morphological characteristics of the structural (or histological) unit have been pointed out by Mall, so that the brief inadequate definition seems sufficient for the occasion.

4. P. 252. The account of the formation of sinusoids is somewhat schematic. We now know that the intercrescence of the vessels and parenchyma offers variations especially in its mode of beginning.

5. P. 259. Huntington's Anatomy of the Peritonæum, etc. (1903), is written entirely from the genetic and comparative standpoint. This excellent work, however, is not a general text-book, and in no sense belongs in the class of manuals criticized in the text. Even Huntington's account of the liver seems to me not to take sufficient advantage of our morphological knowledge, especially as regards the primary connection of the liver with the diaphragm and also as regards the sinusoidal circulation.

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A M. Arthur Shipley
avec beaucoup de remerciements
l'Auteur

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RELAZIONE

SUL IV CONGRESSO INTERNAZIONALE DI ZOOLOGIA

TENUTOSI IN CAMBRIDGE NELL'AGOSTO 1898

DEL PROF. E. ARRIGONI DEGLI ODDI

Libero Docente di Zoologia nella Regia Università di Padova

S. C. del R. Istituto Veneto di scienze, lettere ed arti

VENEZIA
TIPOGRAFIA CARLO FERRARI
1899

PUBBLICAZIONI DELLO STESSO AUTORE

1. Lettera aperta al prof. E. H. Giglioli. Padova, 1884.
2. Nota su di un ibrido di *Turtur auritus* e *T. risorius*. Rovigo, 1885.
3. Sopra una specie del genere *Perdix* nuova per l'Italia. Padova, 1885.
4. Catalogo della Raccolta ornitologica Arrigoni degli Oddi in Caoddo presso Monselice. Padova, 1885.
5. Note ed osservazioni fatte dall'agosto al dicembre 1885, specialmente in riguardo all'emigrazione degli uccelli ecc. *Atti Soc. Ven. Tr.* Vol. III, n. 4. Padova, 1886.
6. Di una femmina adulta di *Passer Italiae* che assunse il piumaggio proprio al maschio. *Atti Soc. Ital. Sc. Nat.* Vol. XXIX. Milano, 1886.
7. Sul Codirosso nel Padovano. *Bollett. del Naturalista.* VI, n. 11, p. 146. Siena, 1886.
8. Osservazioni sul Germano reale. *Ibid.* pag. 147. Siena, 1886.
9. Due ibridi (*Anatidae*) ottenuti in domesticità nel maggio 1885. *Atti Soc. Ven. Trent.* Tom. IV, n. 1. Padova, 1887.
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11. Note ed osservazioni sull'ibrido *Fuligula ferina* × *F. cristata* e sull'ibridismo in generale. *Ateneo Veneto.* Gennaio-febbraio 1887. Venezia. 1887.
12. Sulla colorazione a fascie della coda in alcuni individui giovani di *Merula nigra*. *R. Accad. sc. lett. ed arti.* Vol. III, disp. IV. Padova, 1887.
13. Cicogne e Codirosso. *Riv. Ital. Sc. Nat.* VII. Siena, 1887.
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16. La *Lusciniola melanopogon*, la *Cettia Cettii* e la *Cyanecula Wolfi* nel Padovano. Siena, *ibid.* VIII.
17. Alcuni uccelli anomali del Veneto, con tav. *Atti Soc. Ven. Tr.* Vol. X, fasc. 2. Padova, 1887 (in collaborazione col sig. dott. E. Sicher).
18. Notizie sopra un *Ligurinus chloris* ed una *Alauda arvensis* anomali nel rostro, con fig. *Rivista Ital. Sc. Nat.* IX, fasc. 21-22. Siena, 1889.
19. Contribuzioni all'Avifauna del Padovano nel "Primo Resoconto dei risultati dell'Inchiesta Ornitologica in Italia." Parte I, II e III. Firenze, 1889-91.
20. Notizie sopra un ibrido *Dafila acuta* e *Querquedula crecca*, con tav. col. *Atti Soc. Ven. Tr.* XI, fasc. II. Padova, 1890.
21. Notizie su di un *Synoicus Lodoisiae*, con tav. col. *Ibidem.* Padova, 1890.
22. Su un ibrido *Mareca penelope* e *Querquedula crecca*. *Atti Soc. Ital. Sc. Nat.* Vol. XXXIII. Milano, 1890.
23. Su di un maschio di *Hirundo rustica*, con fig. *Riv. Ital. Sc. Nat.* X, fasc. 5. Siena, 1890.

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Nel marzo dello scorso anno (1898) mi perveniva il gentile invito di partecipare al IV Congresso Internazionale di Zoologia che doveva tenersi nell'agosto in Cambridge sotto l'alto patronato di S. A. R. il Principe di Galles e sotto la presidenza di Sir John Lubbock, un nome troppo caro ai Naturalisti ed ai Dotti di ogni paese. Il programma steso da illustri scienziati era dei più attraenti per invogliare lo studioso, offrendogli il destro di visitare l'Inghilterra, questo paese tanto benemerito anche nel progresso della Storia Naturale. A me poi presentavasi un' occasione graditissima ed utile ad un tempo, quella di associarmi al chiarissimo comm. Spiridione Brusina, Direttore del Museo Zoologico Nazionale e professore nella R. Università di Agram, che recandovisi mi aveva fatto lusinghiera proposta di tenergli compagnia.

Io qui non posso, nè so esprimere con adeguate parole quanto questo carissimo Amico mi fu largo di consigli e di aiuti, di quale paterna bontà mi circondò durante tutto il tempo del nostro viaggio, con quali parole lusinghiere e benevoli mi presentò al Sharpe, al Reichenow, all' Haeckel, al Milne-Edwards ed agli altri suoi amici ed a quali gentilezze fui fatto segno mercè la sua influenza ed i mezzi di cui può disporre una persona così dotta e così giustamente stimata. A lui sento il dovere di rinnovare le più sentite grazie.

Partimmo da Caoddo (Monselice) ai 17 di agosto, in una stupenda giornata, lieto augurio al gradevolissimo viaggio, e sostammo a Milano, ove visitati il prof. Celoria ed il comm. Boito, passammo la serata in casa del comm. Hoepli, sempre ospitale e gentile. Il domani partimmo per Basilea e vi pernottammo, proseguendo il giorno dopo per Calais, ove giungemmo a tarda notte dopo aver attraversato una gran parte di Francia godendo bellissime viste di città, di montagne, di pianure che passavano dinanzi a noi con una celerità vertiginosa e come fossimo davanti ad una lanterna magica di stupenda illusione. Nella mattinata visitammo la città e fra altro il Museo Civico e Raccolte annessevi di Storia Naturale. La Collezione Ornitologica è abbastanza ricca ed interessante, soltanto tutti gli esemplari sembrano come affumicati dall'azione del carbone; osservai un *Accipiter nisus* di disegno regolare ma di una tinta generale cenerino-perlata, un *Buteo vulgaris* in parte bianco, forma di albinismo asimmetrico, con macchie marrone sul gastreo, due *Hirundo rustica*, una delle quali leucocrostica, l'altra normale tranne nelle ali bianche, un'*Anas boscas* interamente di un bianco sudicio, se si eccettui una macchia sulla testa e la coda in parte di un colore normale, una buona serie di *Machetes pugnax*, varie bellissime *Sula* e *Phalacrocorax,* numerose *Anatidae,* buon numero di uccelli esotici, tra i quali notai parecchie specie di *Psittacidi, Picidae* ed una bella *Musophaga violacea.* Più tardi visitammo gli imponenti lavori del porto, vedemmo numerosi brigantini quasi seduti sul fango, tanto la marea era bassa, mentre poche ore dopo essi galleggiavano perfettamente, una pescheria ben fornita di grossi pesci, i *docks* grandiosi, le enormi quantità di legname qua e là accatastate, dozzine di potentissime grue appartenenti alla Camera di Commercio, numerosi vapori commerciali ed il movimento del porto che ci dimostrarono quanto attivi ed importanti ne siano gli scambi. Nella notte con una fittissima nebbia sul *Lord Walsingham* partivamo per Dover. Colà giunti, salimmo immediatamente sul diretto per Londra ove arrivammo in poco più di due ore verso le sei del mattino. Da *Victoria Station* in un *cab* ci recammo a *Euston Station.* Quale strano contrasto fra le vie di Londra, in queste ore mattutine quasi deserte, ove s'incontravano gli spazzini stradali intenti alla spazzatura ed alla inaffiatura e appena qualche *policeman* e qualche raro vagante e la vita tumultuosa che più tardi vi succede, quando la nostra carrozzella trovava disagevole inoltrarsi nel fitto agglomeramento di

veicoli di ogni genere: carri, omnibus, equipaggi, automobili, bicicli e di persone. Meraviglia del pari l' assoluta libertà che vi gode il viaggiatore. Entra in Stazione, prende il viglietto, esce, nulla gli si chiede, consegna semplicemente le sacche al bagagliaio e monta, se non da altri, chiude da sè lo sportello e via, non un fischio, una parola, un grido, nè suono di corno o di trombetta; arrivato a destinazione deve ingegnarsi, perchè nessuno si incarica di lui; se però domanda indicazioni, dal più basso *porter* fino al serio e rigido *station-master* tutti cortesemente si prestano fornendo le più minute informazioni certo nella loro lingua che, strettamente parlata, non riesce facilmente intelligibile. Lo scambiare la stazione d'arrivo è però cosa rara, perchè prima di giungere a ciascuna su due grandi asse ne sta scritto il nome a caratteri cubitali e nella notte tutti i fanali illuminati ne portano l' indicazione. Al viaggiatore inglese deve sembrare strano il sistema delle nostre ferrovie, questo affaccendarsi del numeroso personale, questo continuo vociare, le antifone: *pronti, partenza* ripetute tante volte senza partire mai, gli squilli del corno e che so io, mentre nel suo paese, ove il movimento è di gran lunga superiore tutto procede con una calma ed una serietà davvero più facile ad ammirarsi che a credersi.

Cambridge dista egualmente dalle Stazioni di Euston, San Pancrazio, Liverpool Street e King's Cross circa 58 miglia Inglesi, quindi circa un'ora e venti minuti col treno espresso. Siamo giunti nella storica e vecchia città verso le nove, avendo veduto un paio di volte il serio ed impettito controllore entrare nel vagone dicendo " All tickets, please „ con vera flemma britannica.

Arrivando a Cambridge sembra di entrare in un paese del Medio-Evo, ma chi ha potuto passare qualche tempo nei suoi Collegi ne serba un ricordo così caro ed indelebile da non potersi adeguatamente esprimere a parole.

Splendeva un bellissimo sole e presa una carrozza ci recammo al *Trinity College*, ove il Comitato ci aveva gentilmente allestiti dei magnifici alloggi. Il *Trinity College* fu fondato da Re Enrico VIII nel 1546. Esso ed il *St. John's* sono i due più importanti di Cambridge. Come ad Oxford anche qui i fabbricati della vecchia Università e dei Collegi formano le curiosità delle città e danno loro un aspetto imponente e quanto mai singolare. I Collegi qui sommano a 19 comprendendovi anche due *Public-Hostels*. Essi sono corporazioni indipendenti tra di loro, di fondazione più

o meno antica, aventi ciascheduno le proprie chiese, fabbriche, librerie, laghi, giardini con regolamenti speciali e con un corpo direttivo di cui il capo è detto *Master*, tranne quello del *King's College* chiamato *Provost* e l'altro del *Queen's College President*. Gli studenti, i professori ed il corpo direttivo abitano quasi tutti i fabbricati del Collegio, ciascun Collegio poi contiene un certo numero di aggregati o *fellows*, i quali dopo conseguiti e terminati gli studi possono ancora risiedere nel Collegio, cui sono ammessi o per esame o per titoli onorifici e godono per un termine di sette anni una pensione sui proventi dei fondi del Collegio o sulle rendite dell' amministrazione, il che permette loro di dedicarsi più liberamente agli studi aggiungendo lustro all' Istituto che li ospita. I Collegi sono semplici pensioni, ove lo studente alloggia, prende i cibi e si esercita nei varii generi di sport e di ginnastica, i corsi di studio hanno sempre luogo all' Università. Per abitarvi gli studenti devono sottomettersi a certe regole stabilite dal *Lodging-House Syndacate,* fra queste non ultima quella di non rientrare dopo le 10 di sera e di non uscire prima delle 6 del mattino e tante altre, alle quali i nostri giovani non si adatterebbero, mentre l'Inglese, più serio e più quieto per natura, vi si acconcia volentieri, quando gli sia lasciato il tempo necessario ai suoi prediletti esercizi di sport, che sono svariati e lussuosi, quanto si possa immaginare. In caso di mancanza alle regole, lo studente è severamente punito dalla *Court of discipline* e queste sentenze contemplano la privazione del grado, il confine per un tempo più meno lungo e l'espulsione.

La spesa di uno studente al *Trinity College* si aggira di solito tra le 200 e le 250 sterline per anno, quindi dalle 5300 alle 6700 lire italiane all' incirca. Questa somma tacita tutti i pagamenti dovuti all'Università, al Collegio e tutte le spese personali dello studente durante l'anno, tranne l'epoche delle vacanze. Con economia, la spesa annuale può venir ridotta a st. 175 ed anche 150, ma in tal modo la vita sociale diviene molto ristretta.

Ecco uno specchietto di tali spese:

	A			B		
	L.	sh.	d.	L.	sh.	d.
Diritto dell'Università e del Collegio .	6	0	0	6	0	0
Insegnamento	8	0	0	8	0	0
Conferenze fuori del Collegio . .	1	1	0	—	—	—
Pigione e servizio	10	0	0	7	0	0
Pranzo nella sala con accessori . .	6	10	0	5	10	0
Cucina (nelle camere) . . .	7	0	0	1	10	0
Fornaio e lattaio	3	0	0	1	15	0
Carbone, pulizia, lavatura e stiratura biancheria	3	2	6	2	10	6
	44	13	6	32	5	6
Pagamenti totali all' Università ed al Collegio per tre trimestri . .	134	0	6	96	16	6
Spese personali (negozianti, piccole spese, sottoscrizioni ai Club ecc.) .	100	0	0	75	0	0
Totale	234	0	6	171	16	6

Nel caso lo studente domandi insegnamento privato è richiesta una somma di 9 sterline per ciascuno dei tre trimestri. Se uno studente risiede anche durante le grandi vacanze la spesa addizionale si aggira sulle 17 sterline, escluso l'insegnamento privato. In aggiunta a queste spese annuali, devono farsi le seguenti :

Tassa di Ammissione	St.	5	0	0
Tasse pegli Esami all'Università . .	„	7	7	0
Tassa pel grado di Baccelliere . .	„	12	0	0

Il costo di ammobigliare due stanze nel Collegio è pelle due categorie suddette di circa 30 o 20 sterline rispettivamente ; di tale somma circa metà viene di solito rifusa dall'inquilino che sussegue. Gli alloggi si affittano ammobigliati, solo argenteria, bicchiere, stoviglie per la tavola e la biancheria devono essere provvedute dall' *inquilino.*

Una cauzione *(caution-money)* di st. 15 deve versarsi prima dell'ammissione, ma viene quindi restituita.

Nella sopradetta esposizione nessun aumento vien fatto per qualunque divertimento goduto dallo studente.

In aggiunta a quel qualsiasi emolumento che uno studente può ricevere da qualunque luogo, il Collegio dà una più o meno considerevole assistenza pecuniaria a quelli più capaci o bisognosi; la somma totale a tale scopo contribuita dal *Trinity* è di st. 8000 all'anno, ciò che equivale a più di 200 mila lire italiane. Così le rendite annuali sono per alcuni di questi Collègi favolose; cito le più considerevoli:

Trinity College (1898)	St. 91052.2.0
St. John's College (1897) . . .	„ 40814.1.5
King's College (1898)	„ 38810.9.9
Emanuel College (1898) . . .	„ 15079.5.10 (1).

Poco dopo di essere scesi al *Trinity College* (era il 22 agosto, il primo giorno del Congresso), ci recammo alla *Reception Room (Masonic Hall, Corn Exchange Street)*. Era là che si ritiravano le tessere del Congresso, che si trovavano generalmente riuniti i Colleghi e che dal Comitato, il quale siedeva in permanenza, si potevano avere spiegazioni ed aiuti su quanto faceva d'uopo. Ed io non dimenticherò mai a quali gentilezze ed amabilità fui fatto segno dal sig. *Arthur E. Shipley* del Christ's College, Secretary to the Reception Committee, sempre così forbito e cortese nel disimpegno delle sue funzioni. Nella detta *Reception Room* eranvi salotti per conversare, per scrivere, l'ufficio postale del Congresso ecc. Le riunioni Generali del Congresso si tenevano al *Guildhall* e così quelle di Zoologia Generale, quelle sui Vertebrati alla *Lecture Room of the Physical Laboratory,* quelle sugli Invertebrati, eccettuati gli Artropodi, alla *Lecture Room of the Chemical Laboratory,* finalmente quelle sugli Artropodi alla *Lecture Room of the Comparative Anatomy.* I musei di Zoologia, Geologia, Anatomia, Mineralogia, Botanica ecc., erano aperti tutti i giorni pei membri del Congresso. Come tutti i giorni dalle 1 alle 2 ed alle 7 di sera al *Corn Exchange* si servivano colazioni e pranzi pei Congressisti che vi accorrevano numerosi. Ritirate le tessere, dedicammo la giornata a visitare Cambridge, a scambiare saluti e

(1) Non posso a meno di ringraziare cordialmente l'egr. sig. W. Bateson, ex Fellow del Trinity College e dotto Naturalista, che mi fornì dati e schiarimenti sui Collegi di Cambridge, inviandomi anche vari lavori in argomento.

visite ai Colleghi, fino a che alla sera alle 9 il Congresso fu ufficialmente inaugurato dal Lord Mayor o Sindaco di Cambridge nella grande Sala del Guildhall con un brillante ricevimento e con un sontuoso rinfresco. Trovandosi parimente riunito in Cambridge il Congresso dei Fisiologi vi convennero circa 600 persone, per cui riuscì veramente splendido. Prima il Sindaco sig. Ginn e poi il *Vice-Chancellor* dell'Università diedero il benvenuto, quindi s'intuonarono canti e musiche. Qui si appalesò la piena riuscita del Congresso. Dei 440 iscritti, ben 380 intervennero, fra essi parecchie Signore, fra le quali ricordo con piacere le signore Blanchard, Hubrecht, Simon, Delage, Dollfus e le gentili signorine Blanchard e Vaillant. Fra i Naturalisti noto: Apathy, Bateson, Bemmelen, van Beneden, Blanc, Blanchard, Blasius, Bouvier, Brusina, Collett, Cordeaux, Dautzemberg, Delage, Fauvel, Filhol, Gadeau de Kerville, Gadow, Gibson, Graff, Haeckel, Hartert, Hoeck, Horváth, Hubrecht, Janet, Joubin, Kirby, Lubbock, Lydekker, Macpherson, Maxwell, Milne-Edwards, Möbius, Neumann, Newton, Norman, Perrier, Rothschild, Roule, Salensky, Saunders, Schlumberger, Schulze, Sclater, Selenka, Simon, Stejneger, Studer, Trimen, Tristram, Vaillant, Vejdowsky, Lord Walsingham, Woodward, Wilson, Zograff ecc. Degli Italiani vi trovai soltanto il Carruccio ed il Dohrn; doveva intervenirvi anche il dotto Ittiologo prof. Vinciguerra, ma fu trattenuto in Norvegia per un incarico affidatogli dal R. Governo.

Fu al successivo martedì 23 agosto che alle 10.30 del mattino al *Guildhall* ebbe luogo la vera apertura solenne del Congresso sotto la Presidenza di Sir John Lubbock che fece il discorso inaugurale; poi a nome dell' Università parlò il *Vice-Chancellor*, cui risposero pella Francia il Milne-Edwards, lo Schulze pella Germania, il Marsh pegli Stati Uniti d'America, l'Hubrecht per l'Olanda, il Salensky pella Russia, il Mitsukuri pel Giappone. " Non è necessario, egli disse, che io ricordi a voi Signore ed a questo uditorio i molti fenomeni che sono particolari alla distribuzione delle piante e degli animali. Noi abbiamo detto che gli animali o le piante, che possono appena distinguersi specificamente, si trovano nelle varie regioni della circonferenza della terra. Allo stadio presente del progresso della scienza nel mondo, vi ha una simile specialità nella distribuzione dei centri scientifici. Di gran lunga il più largo numero di essi è ammassato sui due lati dell'Atlantico, intendendo per ciò nell' Europa e nell'America del Nord. Come procediamo dall'Europa

verso Est noi non troviamo alcun terreno indigeno di scienza, fino a che non abbiamo percorso l'intero continente Asiatico. All'estremo limite Est di quel continente, cioè al Giappone vi è un nuovo centro scientifico, che, noi sicuramente lo speriamo, diverrà fiorente col tempo. Sebbene ciò non vi sia noto e famigliare, i saluti che di là io ho portato per voi sono molto cordiali. Noi prendiamo un grande interesse al Congresso Zoologico Internazionale e ci lusinghiamo ch' esso sarà una delle forze più atte ad unire le nazioni tra loro ed a formare una grande fratellanza di tutto il mondo. „ Quindi prese la parola il profess. Alfredo Newton del Magdalene College di Cambridge. Poscia il prof. R. Blanchard di Parigi lesse il suo Rapporto sui Premi istituiti da S. M. l'Imperatore Alessandro III e da S. M. l'Imperatore Nicola II che il Congresso di Mosca ha stabilito si assegnino alla illustrazione dei seguenti soggetti: “ Studi sui Ruminanti dell' Asia centrale „ e “ Studio zoologico e fisiologico su un Gruppo di Invertebrati marini „ e conchiuse che si presentarono soltanto due candidati E. de Pousargues ed il dott. E. Hecht, i quali però produssero eccellenti lavori, che la Commissione Internazionale composta dai signori Milne-Edwards, Blanchard, Jentinck, Sharpe, Studer, Zograff e Flower proposero il sig. E. de Pousargues pel premio di S. M. l' Imperatore Alessandro III ed il sig. dott. E. Hecht per quello di S. M. l' Imperatore Nicola II e che il Congresso approvò all' unanimità queste conclusioni.

Quindi i signori C. Wardell Stiles e P. Lutley Sclater fecero osservazioni sulle leggi di Nomenclatura adottate dalla Commissione Internazionale ed il sig. P. P. C. Hoeck parlò di facilitazioni postali avute per lo scambio degli esemplari zoologici.

Nella stessa seduta su proposta del Segretario i seguenti signori vennero nominati ad unanimità Vice-presidenti del Congresso:

Sig. prof. A. Milne-Edwards di Parigi
„ dott. F. A. Jentink di Leida
„ prof. R. Collett di Christiania
„ prof. E. Haeckel di Jena
„ prof. von Graff di Graz
„ prof. R. Hertwig di Monaco
„ prof. O. C. Marsh di Yale (S. U. d'America)
„ prof. K. Mitsukuri di Tokio
„ prof. W. Salensky di Pietroburgo
„ L. Vaillant di Parigi.

I seguenti signori sono quindi unanimemente eletti Segretari delle Sezioni

Sez. *A.* Sig. dott. P. P. C. Hoeck di Helder (Olanda)
„ *B.* „ prof. Hans Gadow di Cambridge
„ *C.* „ dott. L. Plate di Berlino
„ *D.* „ C. Janet di Beauvais (Francia).

Alle 2.15 pom. vi fu seduta alla Sezione *A. Zoologia Generale* al *Guildhall* sotto la presidenza del prof. J. W. Spengel di Giessen con un uditorio di ottanta persone. Funzionavano per segretario il dott. P. P. C. Hoeck e per vicesegretario il sig. S. Graham Kerr del Christ's College, e vi furono le seguenti letture:

Il prof. K. Mitsukuri " On some Zoological matters in Japan „ tracciando il graduale sviluppo della scienza nel Giappone dai suoi principi che potrebbero dirsi incominciati nel IX secolo. Egli diede il resoconto dello stato della Zoologia nel Giappone, citando la nuova e bella Stazione Zoologica marina di Misaki presso Tokio e la ricchissima fauna marina delle vicinanze.

Il prof. W. Salensky lesse " On Heteroblasty „ sul quale fecero osservazioni il dott. Samassa di Monaco ed il prof. Marco Hartog di Cork, ricordando in appoggio alla teoria del Salensky alcuni lavori del defunto prof. Milne Marshall.

Il sig. J. Stanley Gardner trattò sulle costruzioni degli Atolli, cui seguono osservazioni del sig. G. C. Bourne di Oxford, che raccomanda di studiare gli scogli di corallo dell' Oceano Indiano.

Alla Sezione *B. Vertebrati* si tenne parimenti seduta sotto la presidenza del dott. F. A. Jentink colle seguenti letture:

Del prof. A. Milne-Edwards sugli animali estinti del Madagascar e sulle scoperte fatte pochi anni or sono dai sig.i Bastard e G. Grandidier nei terreni paludosi di quella regione di uccelli del tutto estinti. Il sig. Bastard rinvenne ad Antsirabè le scheletro quasi intero di un grande palmipede che venne descritto dall' Andrews col nome *Centrornis Forsythi*. L' A. dice che lo studio di esso lo dimostrò simile ad un Anserino Australiano, l'*Anseranas melanoleuca*; vi sarebbe quindi speciale affinità fra la fauna del Madagascar e quella delle terre Australi. Grandidier trovò nella parte sud-ovest della grande Isola giacimenti abbondanti di *Aepyornis ingens*. Le zampe rinvenute mostrerebbero che il piede non era atto al nuoto come si arguiva dal fatto di aver

sempre trovato le ossa di questi giganteschi uccelli frammisti a quelle di Ippopotami, di Tartarughe e di Coccodrilli. Il prof. Newton non è dello stesso parere specialmente circa la supposta affinità della fauna di Madagascar e quella dell'Australia e della Nuova Zelanda e W. T. Blanford crede che la fauna di Madagascar sia del tutto propria alla detta regione.

Del prof. O. C. Marsh sull'Importanza dei tipi e sulla loro conservazione.

Del dott. Wolterstorff sugli Urodeli del mondo antico; egli mostra varii acquerelli di Urodeli che fanno parte di un lavoro che comparirà nella " Bibliotheca Zoologica „ e prega i zoologi di favorirgli esemplari vivi o ben conservati di Urodeli dell'Europa meridionale e dell'Asia orientale.

Del sig. dott. I. F. Van Bemmelen: " On Reptilian affinities in the Temporal Region of the Monotreme-Skull. „ Quando studiavo, disse il chiarissimo Autore, il cranio dei Monotremi e lo paragonavo a quello dei Rettili, cercando di trovare affinità coi Sauropsidi, la mia attenzione fu tosto colpita dal fatto che la Regione temporale tanto nell' Ornitorinco che nell' Echidna " is pierced by a canal from before backwards, just over the glenoid cavity for the under-jaw. This canal reminded me vividly of the passage between quadrate and quadratojugal in Sphenodon, and still more of the perforation in (or rather covered by) the squamosum of the Anomodont Psychognathus declivis (Zittel, *Handb. der Palaeont.* p. 358). „

Dopo di aver citato le opinioni del Gegenbaur (*Manual of the Comparative Anatomy of Vertebrates*) e del prof. Seeley (*Proceedings of the Royal Society* for 1896) le di cui vedute il Bemmelen condivide, termina l'importante comunicazione con le seguenti parole: In rapporto con questa osservazione, tre questioni si aprono, indicando tre differenti linee di ricerche che devono necessariamente essere considerate, prima di arrivare a qualche finale conclusione che concerna la reale importanza del foro della regione temporale e che ci trasporta nella questione filogenetica. In primo luogo è assolutamente necessario di venire ad una chiara e sicura spiegazione circa la esatta omologia e la migliore nomenclatura delle ossa componenti la regione temporale e circondanti il canale in questione. Secondariamente è desiderabile di stabilire una completa conoscenza delle parti molli che occupano questo canale. Terzo di ricercare simili passaggi nella regione temporale di altri

animali, specialmente mammiferi. Io spero di poter arrivare a chiari e soddisfacenti enunciati riguardo a questi punti, per ora mi limiterei a far rilevare le considerevoli differenze di opinione circa i nomi reali che devono applicarsi a tali ossa ed in ispecialità poi sulla vera natura dell'osso che forma la superficie esterna del foro temporale. Questo osso ora generalmente ritiensi essere lo squamoso, deduzione alla quale giunsero un tempo Owen e Köstlin, e la quale ci conduce chiaramente alla conclusione che un osso " jugal (malar) „ è intieramente mancante ed inoltre ci rende difficile lo spiegare la grande estensione superiore squamosa dell'osso petroso che succede nel lato interno del così detto squamoso e lo esclude dalle pareti della cavità cerebrale. Owen chiamò mastoide la prolungazione superiore dell'osso petroso; Köstlin lo ritenne una diramazione posteriore temporale dello sfenoide. Sicuramente la sua posizione mostra molta somiglianza col mastoide degli altri mammiferi ma anche coll' " *opishoticum* „ dei " *Testudinata* „. Queste ed altre questioni possono essere soddisfacentemente stabilite dall'investigazione degli stadii giovanili esaminando tutte le suture e facendo esatte comparazioni cogli altri Mammiferi e coi Rettili (1).

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(1) " In connection with this observation, three questions arise, pointing to three different lines of investigation, which must necessarily be worked out, before coming to any final conclusion concerning the real importance of the perforation in the temporal region and its bearing on phylogenetic questions. In the first place it is absolutely necessary to come to a clear und unequivocal understanding as to the exact homologies and the best nomenclature of the bones composing the temporal region and surrounding the canal in question. Secondly, it is desirable to set a full knowledge of the soft parts that occupy this canal and thirdly to search for similar passages in the temporal region of other animals, especially mammals. I hope soon to be able to make a clear and satisfactory statement about these questions; for the present I would confine myself to pointing out the considerable difference of opinion as to the real names that must be applied to these bones, and especially about the true nature of the bone that forms the outer boundary of the temporal perforation. This bone is now generally taken to be the squamosum, a view which was already held by Owen and Köstlin, but which forcibly lead to the conclusion that a jugal (malar) bone is wholly absent, and furthermore involves the difficulty to find an explanation for the large squamous upward extension of the petrous bone, which occurs on the innerside of this so-called squamosum and excludes it

Del sig. I. Graham Kerr sulla Biologia dei Lepidosireni.

Alla Sezione *C. Invertebrati* (eccettuati gli *Artropodi*) si tenne seduta sotto la presidenza del sig. dott. J. E. V. Boas di Copenaghen essendo segretario il dott. L. Plate e vicesegretario il sig. E. S. Goodrich di Oxford, colle seguenti letture:

Del dott. L. Plate sui Chitoni; mostrando come in questa classe di Molluschi, ora creduta in generale la primitiva di tutte fra i Gasteropodi, vi è di gran lunga una maggiore diversità di organizzazione di quella che possa ritenersi giudicando dalla apparenza uniforme, egli seguitò contraddicendo le teorie del prof. Eimen di Tübingen; trattò poscia sopra un singolare protozoo che vive come un parassita nella cavità del mantello dei Chitoni, distruggendo in alcuni casi interamente l'epitelio.

Del sig. E. S. Goodrich " On a new type of Nephridium in the Glyceridae "; egli notò come il Nefridio del " Polychaete Glycera " è di una struttura molto interessante e particolare, disse che in questo verme vi è un " organo ciliato " bene sviluppato in connessione con un " sacco nefridiale " formato di epitelio celomico sopra il quale si stende il vero nefridio; i tre organi strettamente connessi possono chiamarsi il *complesso nefridiale* (" nephridial complex "), esso è situato dalle due bande in fronte del setto in ogni segmento eccetto nei pochi primi; questi organi furono descritti da Ehlers, ma la struttura e le relazioni delle parti non furono affatto comprese; il nefridio non ha apertura interna, come giunge al sacco nefridiale il suo canale si divide e si ramifica, formando una reticella di canali secondari che guidano alla camera dalla superficie esterna o celomica dalla quale emerge il tubo che porta le cellule (" tube-bearing cells ") o solenocite (alquanto simile a quello dei *Nephthys*). Questi *solenociti* consistono di una cellula principale contenente un nucleo, ed un sottile tubo conico nel quale lavora un flagello che proietta nella sottoposta camera nefridiale. Essi sono distribuiti in gruppi di 2 a 5

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from the walls of the brain-cavity. By Owen this upper prolongation of the petrous bone is called the mastoid; Köstlin takes it to be a posterior temporal ala of the sphenoid. Assuredly its position shows much resemblance to the mastoid of other mammals, fut also to the opishoticum of Testudinata. This and other questions can only be satisfactorily settled by the investigation of young stages, showing all the sutures, and by careful comparison to other Mammals and to Reptiles. "

sulla superficie del nefridio. Una diretta osservazione ed un esperimento sembrano mostrare che il nefridio assorbe soltanto i prodotti solubili distrutti del fluido celomico, la funzione dei solenociti è forse l'escrezione dell'acqua.

Del sig. C. F. Rousselet di Londra, il quale diede la descrizione di un suo metodo di preservare e montare i Rotiferi come oggetti duraturi, completamente stesi e nella loro naturale posizione, ciò che non erasi prima d'ora ottenuto; questo metodo consiste nel narcotizzare gli animali colla seguente soluzione:

Soluzione 2 % di cloroidrato di cocaina (3 parti)

Alcool metillico (1 parte)

Acqua (6 parti) che si aggiunge gradualmente goccia a goccia, quindi nell' ucciderli e fissarli con acido osmico molto debole e, dopo di averli lavati, nel fissarli nel formolo al 2 $^{1}/_{2}$ %; con tale sistema l' A. ha fatto una grandiosa Raccolta composta in gran parte di esemplari tipici; l'A. finì ricordando come dopo la pubblicazione della monografia di Hudson e Gesses un gran numero di nuove specie (355) siano state descritte; molte di queste forme null' altro sarebbero se non puri sinonimi di specie già note, ma figurate e descritte con insufficiente esattezza. Egli crede che le collezioni fatte col metodo da lui suggerito siano le più atte ad identificare specie dubbie e ad impedire il danno sempre crescente di dare nomi nuovi a specie già note. Egli suggerirebbe che esemplari montati di tutte le specie descritte di Rotiferi fossero depositate in alcuni Musei, ad es. a Cambridge, ove si potessero esaminare con tutto agio.

Le sopradette preparazioni erano visibili sotto 18 microscopi al Laboratorio Zoologico durante tutte la settimana nella quale ebbe luogo il Congresso.

Alla Sezione *D. Artropodi* si tenne seduta sotto la presidenza del dott. D. Sharp di Cambridge, fungendo da segretario Ch. Janet e da vicesegretario Mr. Cecil Warburton di Cambridge colle seguenti letture:

Del sig. C. Piepers di s'Gravenhage sull' Evoluzione dei colori nei Lepidotteri; cito una conclusione dell'A. sul nuovo soggetto:

Se il fatto è da lungo tempo noto che i bruchi della sunnominata famiglia *(Sphyngidae)* appartenenti alla stessa specie si mostrano sotto differenti colori e che in questo rispetto gli stadii sono variabili, nessuno, per quanto so, ha stabilita la generalità di questo fatto, nè la costante successione di questi colori in un

dato ordine, così da conchiudere che ne origini un fenomeno in cui tali cangiamenti si succedono regolarmente. Così credo di potermi considerare lo scopritore di questo fenomeno (che io osservai in bruchi di altre famiglie) e di potergli dare un nome. Ed il nome che io scelsi è " Evolution of colour „. Poichè quantunque questo fatto consista nella constatazione di un fenomeno non ancora osservato nel colore degli insetti, esso null'altro è invero se non il fatto ben noto dell'evoluzione vale a dire che tutte le forme animali sono costantemente soggette a cambiamenti organici che si operano sotto differenti regole. Così il nome di *Evoluzione di colore* mi sembra il più acconcio (1).

Del sig. Bordage sopra l' " Experiences sur la relation qui existe entre la couleur du Milieu et la couleur des Chrysalides de certains Lépidoptères. „ Le specie contemplate dall'A. sono le seguenti: *Atella phalenta, Euploea Goudotii, Danais chrysippus, Papilio Demoleus* e *P. disparalis*; seguirono osservazioni dei sig.i R. Trimen e H. Caracciolo, rimarcando l' importanza del soggetto.

Del dott. D. Sharp " Some Points in the classification of Insecta Hexapoda „.

Nella serata ebbero luogo due ricevimenti, uno dato dal *Vice-Chancellor* dell' Università al *Downing College Lodge,* il cui parco era sfarzosamente illuminato con festoni di lampade e palloncini di luce elettrica, l'altro con un trattenimento d' organo offerto dal Mann nella magnifica Cappella del *King's College.* In questo fabbricato, che è uno dei grandi capolavori dell'architet-

(1) " If the fact is already long known that the caterpillars of the above-named family *(Sphingidae)* belonging to the same species, appeared in different colours, and that in this respect the different ontogenetical stadia are at variance, no one, as far as I am aware, has stated the universality of this fact nor the constant succession of these colours in a certain order, so as to infer from this the general phenomenon of a regular succession of changes. On this account I think I may consider myself the discover of this phenomenon (which I also observed in caterpillars of other families) and give it a name. And the name I have selected is " *evolution of colour* „ For though it consists in the revelation of a hitherto unobserved phenomenon as far as the colour of insects is concerned, it is indeed nothing else than the well-known fact called evolution, that all animal forms are costantly subject to organic changes in one direction or another-thought they may now and then come to a temporary standstill. Thus the name of " *evolution of colour* „ seems to me to be the proper term. „

tura gotica inglese, è ammirabile quest' organo che onora l'arte italiana, compito sotto il regno di Anna Bolena (1532-36). Vi si notano in differenti parti le iniziali A. R. (Anna Regina) e H. A. (Henricus, Anna) e le armi gentilizie inquartate con quelle d' Inghilterra.

Nel venerdì 24 agosto, alle 10.30 di mattina ebbe luogo la seconda seduta generale sotto la Presidenza del prof. F. E. Schulze. Il prof. Yves Delage di Parigi aprì la discussione sopra la Posizione delle Spugne nel Regno animale. Il punto controverso è di sapere, egli disse, se le Spugne devono essere riguardate come un *phylum* interamente distinto o se esse devono essere riunite ai Celenterati; la ragione principale che impedisce all'oratore di attenersi a quest'ultima opinione si è che le larve delle spugne presentano due tipi cellulari distinti; le cellule a *collaretto* munite di flagelli e istiologicamente di natura ectodermica, le cellule *vitelline* istiologicamente endodermiche e contrariamente a ciò che succede nel resto del regno animale è l'ectoderma che si invagina nell'endoderma. I Zoologi però generalmente non ammettono questo fatto della normale invaginazione che sembra così evidente, essi preferiscono di ammettere che la invaginazione cominci in via normale, dando il nome di endoderma allo strato invaginato e dando poca importanza al fatto che lo strato invaginato mostra i caratteri di ectoderma e *viceversa* (1).

Egli ne concluse che le Spugne si sviluppano dapprima come gli altri Metazoi, ma si separano dai Celenterati allo stadio corrispondente alla *blastula*. Le spugne sono una diramazione principale, quantunque piccola, che si sviluppa direttamente dal tronco di un albero genealogico, indipendentemente dalle diramazioni dei Celenterati e degli altri Metazoi (2).

Seguì il sig. E. A. Minchin di Oxford sulla base della discendenza delle Spugne dal gruppo dei Choanoflagellati, egli concluse che lo sviluppo larvale mostra come le Spugne non deb-

(1) " They prefer to take another course and to admit that the invagination takes place in the normal way, giving the name of endoderm to the invaginated layer, and laying no stress on the fact that the invaginated layer shows the characters of an ectoderm and *viceversa*. „

(2) " The Spongide are a main, although small, branch arising directly from the stem of the genealogical tree, indipendently of the branches of the Coelenterata and of the other Metazoa. „

bano essere considerate fra i Celenterati e che a questo fatto si può arrivare sia nelle larve che negli adulti, per cui sembra che si devano piuttosto considerare derivate dai *Choanoflagellata*.

L' Haeckel quindi prese la parola dicendo come secondo la sua opinione i Celenterati devano comprendere gli Cnidarii, le Spugne ed i Platodi. Tutti sarebbero caratterizzati nel seguente modo: dall'avere un semplice canale gastro-vascolare, sviluppantesi a spese di due fogliette primitive e dall' essere sprovvisti di vasi sanguigni e di celoma. Seguì quindi il sig. G. C. I. Vosmaer di Utrecht, il quale osservò che allo stato delle attuali conoscenze, non possiamo che confessare la nostra ignoranza sulla posizione delle Spugne nel Regno animale. " Io temo, egli soggiunse argutamente, che non sarà molto gradito a quelli che non hanno speciale interesse pelle Spugne di sentire un uomo, il quale è più o meno uno specialista, proclamare la sua ignoranza in argomento, ma io vi prego di credere che ciò è ancora meno gradevole a me stesso „. Ritiene che le Spugne non appartengano ai Protozoi, ciò che però non obbliga ad ascriverle fra i Metazoi, nel senso ordinario della parola (1).

Bütschli, Sollas e Delage sono dell'eguale opinione sotto differenti aspetti, vale a dire che le Spugne appartengano ad un *phylum* separato. La sua opinione è che, dovendo classificarle, vanno poste in un gruppo separato dello stesso valore dei Metazoi, o considerate come Metazoi, ma formanti una classe a parte come i Calenterati, gli Echinodermi ecc. (2).

Seguita il sig. W. Saville Kent che sulla base delle cellule a collaretto può fissare una relazione filogenetica molto simile fra i Protozoi flagellati e le Spugne e raccomanda vivamente lo studio istiologico e biologico delle Spugne su animali viventi e non su esemplari conservati; finalmente il prof. F. E. Schulze crede più conforme al vero il ritenere le Spugne quali Celenterati vicini

(1) " I think we may safely say that Sponges do *not* belong to the Protozoa. This does not involve, however, that Sponges are Metazoa, in the ordinary sense.

(2) " My opinion is, that if we *have* to classify, we must either bring them to a separate group of the same value as the Metazoa, or consider them as Metazoa, but forming a separate Class like Coelenterates, Echinoderms, etc. „

agli Cnidarii. Sembra che l'opinione Delage-Minchin-Kent sia stata accolta con maggiore favore.

Alla Sezione *A. Zoologia Generale* si tenne seduta sotto la presidenza del prof. A. Milne-Edwards colle seguenti letture:

Del prof. E. Haeckel sulla classificazione Filogenetica che aveva già pel primo enunciata nella sua *Morfologia generale* e più recentemente nella *Filogenia sistematica* (1896). I seguenti furono riconosciuti come *true phyla* (cioè gruppi monofiletici, derivanti da un ceppo comune): *Vertebrati, Tunicati, Echinodermi, Molluschi* (tutti derivati dai " Vermalia „ che alla lor volta derivarono dai Platodi), *Cnidaria* e *Spongiae* (1).

A ciò seguirono alcune osservazioni del prof. Marcus Hartog di Cork in riguardo alla posizione dei *Rotatoria*.

Del prof. von Graff di Graz sul Tegumento, il sistema nervoso e gli organi dei sensi dei *Planarii* terrestri.

Del sig. G. C. Bourne sulla Struttura e formazione dello scheletro calcare degli Antozoi.

Del prof. Stephan v. Apáthy " Ueber Nevrofibrillen und über ihre nervös leitende Natur „.

Alla Sezione *B. Vertebrati* si tenne seduta sotto la presidenza di Sir John Lubbock, Presidente del Congresso, colle seguenti letture:

Dei signori professori I. F. Heymans di Gand e Van der Stricht sul Sistema nervoso dell' Anfiosso.

Del prof. I. Cossar Ewart sugli Ibridi di Cavallo e di Zebra, cui seguirono osservazioni dell'on. W. Rothschild, dott. Dixey e P. L. Sclater ai quali tutti replicò il prof. Ewart.

Dei signori A. Kanthack e H. E. Durham di Cambridge sopra un' Affezione parassitaria dei mammiferi causata dalla mosca Tsetse.

Del sig. W. Saville Kent sull'Esistenza della locomozione bipede in certe Lucertole. L'A. considera che la manifestazione di tendenze bipedi in Lucertole di così differente costituzione (gen.

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(1) " The unity of the *Articulata* (= Arthropoda and Annelida) was much debated. The group might be considered to comprise three subphyla, the Crustacea and Tracheata arising from two different groups of the Annelida. These again have originated from Vermalia, i. e. the so-called " Vermes „ after removal of the Annelids and Platoda. Four groups of Vermalia were distinguished, Prosopygia, Frontonia, and Strongylaria-all three developed from Rotatoria *(Trochophora)*. „

Chlamydosaurus, Physignathus, Amphibolurus, Ameiva etc.) sembra giustificare che questo attributo sia stato ereditato con interrotte continuità da certi Rettili i quali presentavano la singolarità distintissima della progressione bipede. Questi Rettili, che erano a quanto sembra più che tutto bipedi, nelle loro abitudini, sono rappresentati da un' importante divisione dei Dinosauri estinti.

Alla Sezione *C. Invertebrati* (eccettuati gli Artropodi) la seduta ebbe luogo sotto la Presidenza del sig. P. Pelseneer di Gand il quale espresse il suo avviso sopra l'uniformità di orientazione delle figure nelle pubblicazioni zoologiche, mostrando di qual grande giovamento ciò sarebbe, se nelle pubblicazioni che trattano sui medesimi oggetti, tutte le figure fossero collocate nello stesso modo, il lato sinistro dell' animale ad es. sempre sul lato sinistro della figura e le stesse abbreviazioni si usassero pegli stessi organi; egli propone che tale questione sia portata all'ordine del giorno del prossimo Congresso; del suddetto professore Sullo sviluppo della *Cenia Cocksi* A. et H.

Del prof. F. Vejdowsky di Praga sulla Fertilizzazione dell'uovo di *Rhynchelmis* e sopra un nuovo organo del senso molto particolare che si trova nella pelle di certe Sanguisughe.

Del prof. S. I. Hickson di Withington sopra le Meduse di *Millepora*.

Alla Sezione *D. Artropodi* si tenne seduta sotto la presidenza di Lord Walsingham, High Stewart dell'Università, colle seguenti letture:

Del prof. A. Dollfus di Parigi sopra la distribuzione geografica degli Isopodi nel Nord dell'Africa dal Senegal fino ad Obock, distinguendo sei gruppi faunistici principali: 1.° Senegal e Isole del Capo Verde; 2.° Isole Atlantiche: Canarie, Madera, Azzorre; 3.° Marocco e Capo Blanc; 4.° Algeria, nelle sottodivisioni seguenti: Algeria occidentale, Algeria orientale, Regione del deserto; 5.° Egitto; 6.° Obock e Gibuti. La fauna delle regioni 2, 3 e 4 è europea d'aspetto, mentre comprende solo generi europei; il massimo sviluppo della fauna degli Isopodi si osserva in Algeria, mentre al Senegal vi è una confusione di forme europee e tropicali ed in Egitto presso il Golfo di Suez ripetesi lo stesso fatto. E qui Lord Walsingham, ringraziando il Dollfus pell'interessante comunicazione, trovò di osservare come la fauna dell'Africa settentrionale ha più affinità con quella dell'Europa meridionale che con le altre di qualunque parte dell'Africa.

Del sig. C. Janet sul seguente argomento: Della costituzione morfologica della testa degli Isopodi giunta allo stato di *imago*. Dopo aver riunite le opinioni ammesse fino oggidì per stabilire a quale segmento embrionale corrispondano le differenti parti dell' integumento, egli cercò di stabilire che lo studio della musculatura fornì un nuovo criterio confermando i precedenti risultati ottenuti e colmando certe lacune; dopo alcune osservazioni preliminari, lo scopo delle quali fu di addimostrare che un muscolo colle sue inserzioni appartiene intieramente ad un solo e stesso segmento, imprese ad esaminare la musculatura della testa e trovò che tutti gli organi interni della stessa, veduti in sezione trasversale, sono costituiti da *gruppi* facilmente riconoscibili. La porzione superiore è formata dal segmento " protocerebrale „ mentre la parte inferiore deriva, in massima parte, dal segmento mandibolare, ma entro questa porzione inferiore trovansi degli organi derivati dal segmento mascellare, che anche include altri organi sviluppati dal segmento labiale. Sir John Lubbock, il dott. Heymons ed il Sharp fecero osservazioni agli enunciati del Janet, plaudendo all'abile e brillante ipotesi.

Del sig. E. Olivier sopra le *Lampyridae*, famiglia di Coleotteri delle Antille composta di 33 specie divise in 6 generi; e finalmente il prof. Bouvier di Parigi parlò sui caratteri esterni dei *Peripatus*.

Nella serata ebbe luogo un suntuoso ricevimento al *Fitzwilliam Museum*, splendidamente illuminato. Il *Fitzwilliam Museum* porta il nome del suo fondatore il Visconte Riccardo Fitzwilliam il quale morendo nel 1816 lasciò le sue preziose raccolte di libri, manoscritti, pitture, stampe ecc. all'Università ed una sostanza di circa due milioni e mezzo di franchi perchè colle rendite si erigesse un Museo, ciò che fu fatto nel 1837. In seguito fu arricchito grandemente con lasciti e con acquisti. Nell' ingresso si ammira la grande statua del Principe Consorte che fu *Chancellor* dell' Università dal 1847 al 1861, opera di Foley. Nel vestibolo numerose sculture ed i busti di Horne Tooke e di E. D. Clarke, professore di Mineralogia dal 1808 al 1822. Altrove notevoli pitture della scuola Fiamminga ed Olandese, capolavori di Rubens, di Rembrandt, di Paolo Veronese, di Tiziano, di Palma il Vecchio, di Millais, di W. B. Richmond, di G. F. Watts ecc. Nella Galleria centrale una collezione di sculture antiche, di bronzi, di sarcofagi egiziani, in altre Raccolte di vasi Greci ed Etruschi e di vetri Fenici, Greci e Romani, molti oggetti Ciprioti, ecc. ecc.

Nel giovedì 25 agosto. Alla Sezione *A. Zoologia Generale* si tenne seduta sotto la Presidenza del prof. A. A. W. Hubrecht con l'argomento " L'Origine dei Mammiferi ".

Aprì la discussione il prof. H. G. Seeley di Londra che si espresse presso a poco così:

Quando alcuni rettili fossili mostrarono di possedere certe ossa dello scheletro quasi identiche a quelle degli uccelli, i rettili e gli uccelli furono raggruppati in una divisione dei Vertebrati detta dei Sauropsidi. Parimenti quando altri rettili estinti mostrarono di avere ossa e denti quasi eguali a quelli dei mammiferi, " the lower mammalia " e gli Anomodonti furono riuniti in un gruppo detto dei Teriopsidi. I rettili detti Iguanodonti furono riguardati come gli antenati degli uccelli e quelli detti Teriodonti come gli antenati dei mammiferi. I diffetti della dimostrazione diedero luogo a differenze di interpretazione. Quando fu descritto il *Theriodesmus* le ossa simili a quelle dei mammiferi mostrarono che esso era un mammifero; ma poscia fu trovato il *Pareiasaurus,* e si notò che il *Theriodesmus* si avvicinava a questo genere nelle sue estremità, e che quindi non era un mammifero, ma un rettile. Egualmente quando fu descritto il cranio imperfetto del *Tritylodon,* si vide che i suoi denti molari erano così eguali a quelli dei mammiferi e così differenti da quelli di qualunque altro rettile noto che questo animale fu ritenuto un mammifero. In seguito si riscontrò che il gruppo dei rettili *Gomphodonti,* rappresentato da più generi, presentava denti tanto complicati quanto quelli del *Tritylodon,* e la struttura del cranio simile a quella dei rettili. Sicchè ora il *Tritylodon* è classificato fra i rettili. Pochi scheletri di questi rettili estinti sono noti. Il *Pareiasaurus* è quasi completo. I *Dicynodonti* sono completi. Il *Cynognathus* è quasi del tutto senza estremità. Questi ed altri scheletri mostrano differenti affinità nelle differenti parti del loro scheletro e dal cranio di *Pareiasaurus* e di *Dicynodon* non trasparisce alcuna delle somiglianze coi mammiferi che si riscontrano in altre parti degli stessi scheletri. Questa differenza fra cranio e scheletro è in armonia con altri rettili fossili, giacchè nessuno ha potuto travedere dal cranio di *Iguanodon* i caratteri del suo " pelvis " e del suo " hind limb " così simili a quelli degli uccelli. Un'altra difficoltà capitale nel determinare questi animali estinti sorge anzitutto dalla diversità dei tipi organici che presentano gli *Anomodonti* e poscia dalla difficoltà di stabilire la portata delle pa-

role mammifero e rettile. Gli *Anomodonti* sembrano mostrare affinità cci tipi più bassi dei rettili così come con più tipi di mammiferi; di più tengono parecchio degli estinti rettili. La forma del cervello, se ciò fosse utile, mostrerebbe affinità dello stesso valore. L' espansione laterale degli emisferi cerebrali e la loro estensione all' indietro sopra altre parti del cervello sono forme proprie ai mammiferi; ma esse non si riscontrano negli *Anomodonti.* La cavità cerebrale è imperfettamente nota e non vi è sicurezza se il cervello la occupasse. La sua parte posteriore in certi *Dicynodonti* è più profonda che larga alla base, i suoi lati piatti ed il bordo superiore " is a sharp ridge „. Come forma è più somigliante al modello della cavità cerebrale in certi *Dinosauri* che alla corrispondente regione in alcun rettile o mammifero vivente. Un tipo Americano affine ai Teriodonti Africani mostra di avere la cavità cerebrale compressa da un lato all'altro, e simile a quello del Belodon e di alcuni Dinosauri. Gli *Anomodonti* non sono però intermedii fra i rettili ed i mammiferi nella forma della loro cavità cerebrale ed in questo carattere sono più intermedii fra i rettili e gli uccelli che non lo siano i *Dinosauri.* Seguita dimostrando le differenze di struttura fra gli Anomodonti ed i mammiferi primitivi e conclude: Gli Anomodonti non sono i progenitori dei mammiferi, ma un gruppo collaterale e strettamente affine. Il comune progenitore di entrambi deve ricercarsi in roccie più vecchie del Permiano, forse negli strati Siluriani o Devoniani. I resti finora scoperti nel Permiano mostrano un così intimo ravvicinamento dei più alti Rettili ai più bassi Mammiferi, che è ragionevole di credere che l' intervallo fra essi è già ridotto così piccolo che può essere obliterato da future scoperte. Finora peraltro non vi è alcuna prova che il tipo di cervello proprio ai mammiferi sia stato trovato in questi mammiferi (1).

(1) " Anomodonts are not the parents of mammals, but a collateral and closely related group. The common parent of both may be sought in rocks older than Permian, perhaps in Silurian or Devonian strata. The remains thus far discovered in Permian rocks show so near an approach of the higher reptiles to the lower mammals, that it is reasonable to believe that the interval between them is now so small that it may be obliterated by future discoveries. There is however thus far no evidence that the mammalian type of brain had come into existence in these mammals. „

Seguì il prof. Osborn di Nuova York. Egli dice che in primo luogo i mammiferi possedono il potere di rapidi adattamenti alle condizioni della loro vita. Vi furono quattro centri principali di " adaptative radiation ", il migliore dei quali è quello dell'Australia, ove i marsupiali hanno acquistato forme che fra i mammiferi placentati sarebbero diverse in differenti ordini. " The starting point of each adaptative radiation has been a small, unspecialized land mammal. "

Finalmente è probabile che " the ancestral mammal " fosse insettivoro o omnivoro. Ricordando questi principi noi possiamo tracciare " the line of mammalian descent backward ", essa ci porta al periodo Giurassico, quando i mammiferi erano tutti piccoli ed appartenevano a tre gruppi: gli insettivori primitivi, che furono riguardati quali marsupiali, quantunque ciò non sia affatto sicuro, secondo i " Multituberculates " che sono probabilmente Monotremi primitivi, terzo i Marsupiali. Rovesciando l' ordine di ricerca, il prof. Osborn si riferisce al fatto che nel periodo Permiano vi sono tre gruppi di rettili, uno dei quali è sorprendentemente simile ai mammiferi in alcuni de'suoi caratteri, per cui siamo tentati di unire la sezione erbivora degli Anomodonti ai Monotremi. Egli pensa, però, che i molti punti accentuati di somiglianza fra questi rettili ed i mammiferi siano dovuti al parallelismo, simili caratteri essendo stati acquistati indipendentemente. Egli opina come il prof. Seeley che gli Anomodonti conosciuti non sono i diretti antenati dei mammiferi, ma una linea collaterale. Ma non ne approva le conclusioni quando crede ad un antenato molto più primitivo e comune sì ai mammiferi che agli Anomodonti perchè egli ritiene che in seguito si scoprirà come il vero antenato dei mammiferi sia un terzo nuovo e meno particolare sotto-gruppo degli Anomodonti. L'Hubrecht, però, ha mostrato che l'uovo dei mammiferi è, come carattere, piuttosto " amphibian " che " reptilian " e nel caso si dia molta importanza a ciò, i mammiferi potrebbero essere discesi da qualche rettile che riteneva certi caratteri anfibi.

Il prof. Marsh dice che la questione in parola non è nuova, ma non meno interessante e difficile. L'origine degli Uccelli, dei Rettili, degli Anfibi e dei Pesci realmente la precede ed offre minori difficoltà di soluzione. La soluzione, secondo lui, appartiene al futuro. I mammiferi, nel modo nel quale oggi ci sono noti sono classificati da per se stessi, e contengono ancora così diversi gruppi

che possiamo chiaramente domandarci se tutti avranno avuto una origine comune. Il tentativo di accertare donde essi derivarono è come se noi cercassimo se essi ebbero varii ceppi di origine e, se ciò fosse accertato, sarebbe in parte spiegata la grande varietà che essi presentano. È però evidente che alcune delle caratteristiche mostrate dagli animali come il cuoio capelluto, il sistema circolatorio e le glandole mammarie non possono essere cercate nelle forme fossili. La struttura ossea può ora soltanto essere considerata nei primi mammiferi e negli altri vertebrati e in questo unico fatto di rassomiglianza si può vedere se differenti punti sono " connected genetically „. Esamina e paragona specialmente il cranio di animali fossili e più specialmente i denti, l' osso squamoso, il quadrato, i condili occipitali e la mascella inferiore e conclude: Avendo così mostrato che colle nostre presenti conoscenze noi non possiamo trovare l' origine dei mammiferi fra i rettili estinti ora noti, e che tentando ciò noi siamo ben lontani dalla loro vera linea di origine, altro non ci resta che di indicare una nuova direzione nella quale la ricerca sembra mostrarsi promettente. Fin dal 1876, quando Huxley mi visitò a New Haven, noi discutemmo la probabile origine tanto degli Uccelli che dei Mammiferi, ed io fui grandemente impressionato dall' idea che i mammiferi fossero derivati da antenati con due condili occipitali e che questi fossero indubbiamente gli Anfibi primitivi. Io ho sin d'allora diligentemente ricercato gli antenati degli uccelli fra i rettili più antichi e la mia ricerca mi fece sperare in qualche successo, ma ciò è un semplice problema che si può paragonare all' origine dei mammiferi che noi ora stiamo studiando. In varii colloqui avuti con Francis Balfour, nel 1881, " at the York Meeting of the British Association „ noi discutemmo le stesse questioni e fummo d'accordo che la migliore soluzione cui potremmo arrivare si farà solo coll'aiuto riunito dell'Embriologia e della Paleontologia. Egli si offerse di studiare " the young stages „ delle forme recenti ed io mi accordai di considerare i fossili per altre conclusioni. La sua morte improvvista, avvenuta poco dopo, distrusse le promesse ricerche e la scienza soffre ancora di tanta perdita. Se Balfour fosse vissuto, egli ci avrebbe potuto dare oggi la soluzione della grande questione e la presente discussione riescirebbe inutile. Gli Uccelli come i mammiferi hanno più spiccatamente sviluppati certi caratteri che non i rettili, e così le due classi sembrano avvicinarsi l'una all'altra. Io dubito, però che essi

siano stati " connected genetically „ o almeno in epoca molto lontana. I Rettili quantunque d'ordine molto più basso degli uccelli, assomigliano ai mammiferi sotto varii rapporti ma ciò può essere soltanto " an adaptive likeness. „ Entrambe queste due classi possono essere state composte di gruppi complessi di affinità soltanto lontane. Essendosi entrambi sviluppati secondo linee eguali essi mostrano vari punti di rassomiglianza che possono facilmente essere scambiati per indicazioni di vera affinità. Negli anfibi, e specialmente nelle forme più antiche, vi sono indizii di vera parentela tanto coi Rettili che coi Mammiferi. Mi sembra, quindi, che in alcune piccole forme primitive, tanto vecchie quanto il Devoniano, se non ancora più antiche, noi possiamo trovare eziandio la chiave del grande mistero dell'origine dei Mammiferi.

Il prof. Haeckel dice che egli discusse il problema 32 anni fa con Huxley e Lyell, ed il primo allora sosteneva fortemente " the polyphyletic origin „ dei mammiferi placentati, i gruppi carnivori ed erbivori sarebbero discesi rispettivamente da marsupiali erbivori e carnivori. Quest'opinione ora non può sussistere e l'A. crede che le differenti serie di mammiferi placentati convergano così strettamente che essi debbano avere per comune antenato un marsupiale.

Il prof. Adam Sedgwich di Cambridge richiama l'attenzione sul fatto cho l'Embriologia, sul cui aiuto i precedenti oratori sperano tanto, diede fin'ora ben poca luce se non nulla affatto per la soluzione del problema. Riguardo agli organi, che nell'ordinarie vedute dell'evoluzione, devono essersi alterati in un periodo recente come le ali degli uccelli, le estremità di alcuni serpenti, i piedi degli ungulati, e l'assenza di denti sugli uccelli viventi, non esiste segno di un più completo sviluppo nello stato embrionale. Esaminando le relazioni fra animali estinti e viventi, cercando di classificarli e rammentando che tutte le grandi classi del regno animale, che poterono essere conservate quali fossili, fecero già la loro comparsa nelle roccie Paleozoiche più inferiori, si dovrebbe essere colpiti dal fatto che vi fu molto a dire sul fenomeno che la più gran parte del cangiamento evoluzionista ebbe già origine nel tempo precambriano prima del periodo fossilifero (1).

(1) " One could not be struck by the fact that there was much to be said for the view that the greater part of evolutionary change had already taken place in precambrian times before the fossiliferous period. „

Se quest'opinione è giusta, e la possibilità di ciò può avvenire, la parte principale dell'evoluzione degli organismi dovrà cominciare sotto condizioni del tutto differenti a quelle ora esistenti e rimanere per sempre a noi sconosciuta. Noi dovremmo quindi considerare i fossili in particolarità quali connessi collateralmente piuttostochè progenitori di forme viventi e l'evoluzione durante il periodo fossilifero in principalità consistita in un processo di estinzione di varietà meno adatte, che nella formazione di importanti tipi nuovi (1).

Il prof. Hubrecht, replicando al Sedgwick, disse che l' Embriologia in molti casi ebbe a distruggere piuttostochè costruire. Egli non condivide le idee di Haeckel, dacchè Hill ha dimostrato che nel Perameles, genere di Marsupiali Australiani, fu rinvenuta una placenta distinta. Egli predice che la grande battaglia nel futuro si combatterà sulla questione se i mammiferi sieno discesi da antenati ovipari.

Alla Sezione *B. Vertebrati*, seduta tenuta sotto la presidenza del sig. W. Saville Kent, colla lettera del prof. Mac Intosh W. C. di St. Andrews, Fife, sulle Ricerche scientifiche fatte in un periodo di 12 anni (1886-97) per rilevare l'effetto del *Trawling* nelle acque della Scozia.

Dopo mezzogiorno alla *Senate House* si conferirono i *Gradi onorari* (Honorary Degrees) ai seguenti personaggi insigni dei Congressi di Zoologia e di Fisiologia:

1. Henry Pickering Bowditch,
2. Anton Dohrn,
3. Alphonse Milne Edwards,
4. Camillo Golgi,
5. Ernst Haeckel,
6. Ambrosius Arnold Willem Hubrecht,
7. Hugo Kronecker,
8. Willy Kühne,
9. Etienne Jules Marey.

(1) " We should then have to look upon fossils in the main as being rather collaterally than ancestrally connected with living forms, and evolution during the fossiliferous period as having consisted mainly of a process of extinction of the less fit varieties than of the formation of important new types. „

Nella sala affollatissima su di un apposito spazio presero posto i futuri decorati e le prime Autorità dell'Università e dopo che il *Public Orator* ebbe pronunciato il discorso di elogio, ciascuno venne decorato colle cerimonie di uso.

Riproduco a titolo di curiosità, il discorso col quale il *Public Orator* (dott. Sandys) presentò il prof. Anton Dohrn, direttore della Stazione Zoologica di Napoli:

" E Germanis quidam oriundus, patris iucundi filius laudem ideo maximam est adeptus, quia Italiae in litore hospitali, orbis terrarum in sinu amoenissimo, vivarium oceani spoliis reservatum gentibus patefecit, quod quasi aquarum castellum appellaverim, unde doctrinae rivuli in omnes terras late diffluxerunt. Vivarii illius conditorem inter hospites nostros diu numerevimus; eidem alumnos nostros animo laeto commendavimus; ab eodem scientia varia instructos animo grato rursus accepimus. Ipse animalium in partu praesertim explorando laboris immensi prodigus, neque minorem quam in vivario illo condendo fortitudinem ostendit, neque fortunam minus prosperam expertus est. Per totam certe vitam feliciter confirmavit verba ab ipso Plinio, historiae naturalis auctore locupletissimo, vitae suae in die novissimo prope Neapolim pronuntiata: " fortes fortuna iuvat ".

Duco ad vos *Antonium Dohrn* ".

La funzione fu quanto mai fantastica. Il corteo quindi uscì per ordine gerarchico sciogliendosi nella *Senate House Yard*.

Più tardi ebbe luogo un *garden party* nello spazioso Giardino Botanico dell' Università. Colà vennero serviti rinfreschi mentre suonava la *Imperial Blue Hungariam Band*. Nelle vastissime serre ammirammo fra le piante rampicanti la *Araujа cericifera*, celebre per la tendenza che hanno i suoi fiori a prendere gli insetti, uno splendido esemplare di *Todea barbara* dell'Australia e fra le felci inglesi, la *Gymnogramma leptophylla*, la *Daurallia pyxidata*, il *Platycerium alcicorne* e fra le esotiche il *Lygodium scandens* e lo *Psilotum Triquetrum* ecc. Fu qui che mi si offerse la ambita ed onorevole occasione di trattenermi a lungo col venerando prof. Alfredo Newton dell'Università di Cambridge, ornitologo di fama mondiale.

Nel venerdì 26 agosto alle 10.30 seduta generale del Congresso al Guildhall sotto la presidenza di Sir John Lubbock, presidente del Congresso.

Il prof. Ernst Haeckel vi tenne una conferenza sullo " Stato

delle nostre conoscenze circa la discendenza dell'Uomo „. La posizione dell' Uomo nella zoologia è ancora, come disse l' Huxley, la questione delle questioni pel genere umano e se questa osservazione fu esatta nel 1863, lo è più oggidì, come osservò l' A.: Alla fine del 19° secolo (il periodo delle Scienze naturali) il ramo che più si sviluppò fu la zoologia. La *origine monofiletica di tutti i mammiferi* dai monotremi fino all'uomo non è una vaga ipotesi, ma un fatto positivamente stabilito. Tutti i mammiferi viventi od estinti che noi conosciamo discendono da una sola forma comune di progenitori che visse nel periodo Triassico o Permiano e questa forma deve essere derivata da qualche *Rettile* Permiano e forse Carbonifero (affine ai Progonosauria od ai Teriodonti) come questo dal Carbonifero *Amphibian* (Stegocephalia). Essi discendono dai pesci Devoniani e questi ancora da vertebrati inferiori (1).

Il fatto più importante si è che l' Uomo è un Primate (2) (Linneo) e che tutti i Primati (tutti i Lemuri, tutte le Scimmie e tutti gli Uomini) discendono da un ceppo comune (Huxley). La zoologia può essere superba di aver constatato ciò sulle teorie di Lamark (1809) e di Darwin (1859). L'immenso progresso che si è fatto sarà una delle più grandi conquiste del pensiero umano (3).

S' aprì quindi una discussione fra l' Haeckel ed il Rev. T. R. R. Stebbing sulla età della Terra stabilita da Lord Kehin in 25 milioni di anni; vi seguirono poi le letture del sig. E. Dubois di 's Gravenhage " Remarks on the brain-cast of *Pithecanthropus*

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(1) " At the end of the nineteenth century, the age of " natural science „ the department of Knowledge that has made most progress is zoology. The *monophyletic origin of all Mammalia* from the monotremata upwards to man is at present no more a vague hypothesis, but a positively *established fact*. All the living and extinct mammalia, which we know, are descended from one single common ancestral form, which lived in the Triassic or Permian period, and this form must be derived from some Permian or perhaps Carboniferous *Reptile* (allied to Progonosauria and Theriodontia) as well as the latter from Carboniferous *Amphibian* (Stegocephalia). These latter descend from Devonian fishes and these again from lower vertebrates. Much more difficult is the question of the origin of the great vertebrate-stern, and its desunt from Invertebrates. „

(2) " *Man is a member of the Primate-Order.* „

(3) " The work done in the present century by Lamark and Darwin will in all future times be considered one of the greatest conquests made by thinking man. „

erectus „ e di Mr. L. H. Duckworth di Cambridge “ Notes on Anthropoid Apes „ e quindi il sig. E. I. Marey di Parigi richiamò l'attenzione del Congresso sui fenomeni di locomozione animale, parlò dei suo lavori in argomento e conchiuse di quale interesse saranno per le constatazioni anatomiche le osservazioni biologiche fatte con buon metodo.

Alle ore 2 pom. alla Sezione *A. Zoologia Generale* si tenne seduta sotto la presidenza dello stesso sir John Lubbock, Presidente del Congresso, colle seguenti letture:

Di Sir H. Maxwell di Londra sulla Recente legislazione sulla protezione degli Uccelli in Granbretagna. L' A. parlò dei Wild Birds Protection Acts (1880, 1894 e 1896) vigenti in Inghilterra e che sembrano aver dati buoni risultati circa la conservazione di molte specie. Credo inutile dilungarmi su questo soggetto, tanto più che quelle disposizioni legislative, ottime per la Gran Bretagna, non lo sarebbero egualmente per noi.

Del prof. Mac Bride di Montreal sull'origine degli Echinodermi.

Di Mr. J. A. Harvie-Brown sopra “ A Correct Colour Code, or Sortation Code in Colours to serve for Mapping the Zoogeographical Regions and Sub-Regions of the World, and also to be of use as an Eye-Index for Librarians „. Dopo aver discusse le note divisioni proposte da Sclater, Wallace, Heilprin, Allen e Beddard egli pone avanti le seguenti:

1. Regno Artico
   Regione Paleartica
   Regione Neartica
   Regione Neotropica
   Regione Etiopica
   Regione Orientale
   Regione Australe
2. Regno Antartico.

Parla quindi della “ Nomenclature of Colours „ di Werher, della “ Chromotaxia seu Nomenclator Colorum „ di Saccardo di cui accetta parecchie deduzioni, adottando più innanzi i principali colori della “ Nomenclature „ di Hay ecc.

Alla Sezione *B. Vertebrati* si tenne seduta sotto la Presidenza del prof. Ernst Haeckel colle letture:

Del prof. A. A. W. Hubrecht sui Processi ematopoetici nella Placenta.

Del prof. H. Osborn sopra un Iracoide fossile del Pliocene inferiore dell' Isola di Samo e sulla ristaurazione dei Vertebrati esistenti nel Museo Americano di Storia Naturale.

Del prof. L. Vaillant sulla struttura speciale delle spine negli Apogonini e qualche altro Acantottero.

Del prof. W. Salensky sullo sviluppo dell' *Ichtyopterygium.*

Del sig. E. de Pousargues di Parigi che mostrò disegni dei due sessi ed in varie età del *Rhinopithecus Bieti.*

Del prof. A. Milne Edwards sopra l'*Aepyornis* e diversi altri uccelli estinti dell' isola di Madagascar.

Del prof. A. Carruccio di Roma sugli uccelli della Collezione Regionale Romana, Raccolta degna di vero encomio.

Del prof. Ramsay Wright " On the so-called uterus, masculinus of the Rabbit ".

Del sig. H. Nitsche di Tharandt sulle Corna dei Cervi ed in generale sui Ruminanti.

Alla Sezione *C. Invertebrati* (eccettuati gli Artropodi) la seduta fu presieduta dal sig. C. Wardell Stiles dell'Ambasciata Americana di Berlino colle letture:

Del dott. F. Zschokke di Basilea sugli Entozoi nei Mammiferi senza placenta.

Dei signori M. Caullery di Lione e F. Mesnil " Les formes épitoques des Annéllides et en particulier des Cirratuliens ".

Del prof. E. L. Mark di Cambridge, Mass. U. S. A sopra un nuovo tipo di Attinie.

Di F. W. Harmer di Norwich " On the range in time and space of *Fusus (Neptunea) antiquus* and its allies. "

Del signor A. E. Malard-Duméril di Saint-Vaast-la Hogue, Manche, anche a nome del prof. E. Perrier di Parigi sulle Relazioni da stabilirsi fra i differenti Laboratori Marittimi per studiare alcune questioni di Biologia generale che concernono esseri marini.

Del prof. W. Schewiakoff di Pietroburgo sopra un nuovo metodo di colorare i cigli, i flagelli ed altri organi motori dei Protozoi.

Del sig. Pierre Fauvel di Angers: " Les stades post-larvaires des Arénicoles ".

Del sig. prof. Louis Roule: “ La structure de la larve Actinotroque des Phoronidiens „.

Nella serata al *Trinity College* ebbe luogo il banchetto di chiusura del Congresso. V' intervennero 225 persone per sottoscrizione, nessuna signora, opponendovisi i regolamenti dell'Istituto. Queste però furono ammesse al momento dei numerosi ed entusiastici brindisi nelle Tribune della Hall. Vi regnarono la massima cordialità e simpatia internazionale e fu degno congedo alle cordiali, splendide, indimenticabili accoglienze che ricevemmo a Cambridge.

Nel sabato 27 agosto nella mattinata alle ore 9 $^1/_2$ sotto la presidenza di Sir John Lubbock vi fu la seduta di chiusura per stabilire anche il tempo ed il luogo del 5° Congresso Internazionale di Zoologia. Per incarico dei delegati Tedeschi il prof. K. Möbius di Berlino invitò a tenerlo nel 1901 in Germania.

La proposta fu approvata e fra vivi applausi ad unanimità. Quindi Sir John Lubbock propose di votare ringraziamenti all'Università, ai Collegi di Cambridge, al Sindaco ed al Consiglio Municipale per le gentili e festose accoglienze avute e tutti approvarono. Sorsero poscia il prof. A. Newton per ringraziare a nome dell' Università ed il Sindaco pel Municipio e da ultimo il prof. Yres Delage si fece interprete dei sentimenti di riconoscenza della Società Zoologica di Francia verso il Congresso, proponendo un voto di plauso all'instancabile e valoroso Presidente ed ai diligenti Segretari. Su proposta del prof. F. Jeffrey Bell di Londra fu nominata una Commissione composta dei signori Evans, Mark, Pelseneer e Schulze per determinare le regole della terminologia e così il Congresso suggellò i suoi lavori, aggiornandosi a Londra atteso dalla più cortese accoglienza.

Il Comitato aveva avuto la felice idea di apprestare nel Laboratorio di Zoologia una specie di Museo, ove i Congressisti potevano esporre i pezzi e le preparazioni anatomiche che avevano servito di tema ai loro lavori. Fu un giusto proposito dacchè ognuno poteva dopo aver udito le teorie e le varie osservazioni svolte dagli Autori, portarsi nel Laboratorio e *de visu* constatare ciò che torna difficile ritenere da una semplice spiegazione orale. E nel vasto salone dell' Istituto Zoologico tali preparati erano esposti in gran copia sotto a non meno di centocinquanta microscopii. Fra i pezzi più importanti esposti noterò:

Sig. G. Apathy: Cellule e fibre nervose,
„ R. Assheton: Segmentazione dell'uovo di Montone,
„ W. Bateson: Raccolte di farfalle e tavole della distribuzione delle varietà della *Pararge egeria*,
„ J. S. Budgett: Sviluppo della *Phyllomedusa hypochondrialis*,
„ M. Caullery et F. Mesnil: Parassita delle Gregarine,
„ H. E. Durham: Parassiti delle Tsetse,
„ H. H. Field: Esposizione Bibliografica,
„ S. Gardner: Madrepore dell' Isole Fidji,
„ E. Gibson: Nuovi Nematodi delle isole Fidji,
„ E. Haeckel: Collezione di Radiolarie,
„ F. Harmer: *Cephalodiscus, Tubulipora plumosa* e *Crisia*,
„ R. Heymons: Sviluppo di *Scolopendra* e *Grillotalpa*,
„ A. A. W. Hubrecht: Processo ematopoietico nella placenta dei Mammiferi,
„ J. Graham Kerr: Sviluppo di *Lepidosiren* dell'uovo alla età di 18 mesi (32 splendide preparazioni), adulti e scheletro,
„ J. Lister: Foraminifere,
„ E. W. Mac Bride: Sviluppi di *Asterina gibbosa*, d'*Amphiura squamota* e d' Anfiosso *(Amphioxus)*; oltre molti altri.

Il Comitato e le Autorità dell' Università agevolarono in ogni guisa la visita dei ricchissimi Musei.

Dirò brevi parole sulle Collezioni Ornitologiche delle quali specialmente m' interessai. Ebbi ad ammirarle nella *Bird Room* nel Museo di Zoologia al piano superiore dell' Università; principiarono con la Raccolta Generale dell'illustre zoologo Guglielmo Swainson, ove conservansi in gran parte gli esemplari tipici da lui descritti; pervenne all'Università nel 1840 per merito del defunto Sir George Paget. Vi si aggiunsero in seguito le Collezioni di Uccelli Australiani fatte dal capitano Blackwood, R. N. e quelle della *Philosophical Society* riunite nel 1865, nonchè una serie quasi completa di Uccelli Inglesi. Nel 1867 questo Museo fu arricchito della grandiosa raccolta generale di H. E. Strickland con 6000 esemplari riferibili a 3000 specie, di quelle di A. Smith del Sud Africa, di J. Hepburn della California e Columbia Inglese, della numerosa del Madagascar e delle Isole Mascarene del compianto Sir Edward Newton, del Magdalene College e delle numerose serie della Nuova Zelanda dovute ai signori James Hector, Capt. Hutton e Sir Walter Buller e delle isole Sandwich largite dalla

generosità del simpatico naturalista Scott B. Wilson, finalmente di una magnifica collezione di Uccelli del Paradiso raccolti dal dott. H. Guillemard del Caius College durante il viaggio della *Marchesa*. Fra le rarità vedute ricordo delle *Meliphagidae* la *Chaetophila angustipluma* da Haway nell'Isole Sandwich, avuto nel 1889, uccello ritenuto estinto, e la *Drepanis pacifica* (Gmelin) avuto nel 1859, poi l'unico esemplare di *Chloridrops kona* raccolto e descritto dal Wilson nel 1887 e varii bellissimi Uccelli del Paradiso, una splendida *Alca impennis,* due uova e lo scheletro, della Raccolta Inglese un maschio di *Anas boscas* e *Dafila acuta* ancora della Collezione Yarrel ed un maschio di *Mareca penelope* e *Dafila acuta* acquistato sul Leadenhall Market a Londra e varii *Syrrhaptes* di provenienza inglese. Della ricchissima Raccolta di resti fossili di Uccelli estinti ricorderò i due scheletri quasi completi di *Pezophaps solitaria* di Rodriguez e di *Didus ineptus* del Madagascar, due pezzi di inestimabile valore.

Chiudo con un saluto cordiale e riconoscente all'illustre prof. dott. Hans Gadow F. R. S. *(The Strickland Curator)* che con somma amabilità ebbe un giorno a mostrare al prof. Brusina, al prof. Möbius, direttore del Museo di Berlino, ed a me i soggetti più interessanti del suo ricco Museo.

Meravigliati per la suntosità degli edifici, per la magnificenza dei Musei abbandonammo a malincuore Cambridge, memori delle belle giornate così presto fuggite in una convivenza istruttiva, fra amicizie che appena contratte ci sembrarono antiche, in quei costumi serii, a noi pur troppo ignoti e dei quali l'urbanità permette alle *young ladies* di andare tranquillamente a diporto fino a notte inoltrata contegnose sempre e dovunque rispettate.

Addio classica Cambridge, un saluto ossequente e cordiale ai professori Alfredo Newton M. A. ed Hans Gadow, ai signori W. Bateson e Adam Segdwick M. A. ed un' affettuosa stretta di mano al sig. A. E. Shipley M. A. che ci ha prodigato con vera assiduità la sua opera intelligente ed utilissima.

Alle 11 antim. di sabato 27 agosto partimmo dal simpatico soggiorno di *Trinity College* per Londra. Là attendevaci la Società Zoologica dandoci il ben venuto con un ricevimento nei suoi giardini che durò dalle 16 alle 19.

Facevano gli onori di casa (come suol dirsi) l'illustre segretario il sig. P. L. Sclater, un'antica e cara conoscenza ed il sig. F. E. Beddard. Questo ricevimento fu sontuoso e gli invitati

avrebbero potuto a tutto loro agio visitare le ricche raccolte di animali viventi, se una dirotta pioggia non l'avesse impedito, guastando nel bel mezzo il geniale convegno. Situati nel lato Nord di Regent's Park i giardini della Società Zoologica di Londra sorsero prettamente per iniziativa privata e insieme a quanto racchiudono senza alcun sussidio Governativo e si ritengono i più importanti d'Europa e del mondo. La fondazione ebbe luogo nel 1829 sotto il titolo di " The Zoological Society of London ", lo scopo, di far progredire la scienza zoologica e di introdurre e far conoscere nuovi o curiosi soggetti del Regno Animale. I principali suoi fondatori furono Sir Humphry Davy e Sir Stamford Raffles; qui si può ben asserire che mai si lesinarono i mezzi a speciali incaricati spediti nelle varie parti del globo alla ricerca di animali fra i più curiosi ed interessanti. Attuale Presidente è Sir William Henry Flower, allora ammalato, e segretario l'illustre P. L. Sclater, un nome tanto noto e così caro a tutti i Naturalisti. I vastissimi giardini sono dall' " Inner Circle " e dal " Regent's Canal " divisi in tre parti che si chiamano rispettivamente " The South Garden " " The Middle Garden " e " The North Garden ", essi sono uniti da un tunnel sotto l' " Inner Circle " e da un ponte sopra il " Regent's Canal ". Il numero degli animali generalmente esposti è di circa 2500 e quello annuo dei visitatori dei giardini supera la media dei 600 mila. Pagano per l'ingresso 1 sh. gli adulti e 6 d. i ragazzi, ed al lunedì 6 d. indistintamente, la domenica sono chiusi, eccetto che pei Soci ed i loro Amici o per quelli che sono muniti di speciali biglietti. Se la memoria non mi tradisce, oltre tremila sono i Membri della possente Società, pagano una tassa di ammissione di st. 5 ed un'annuale di tre, od una volta tanto 35 sterline, da queste cifre e dalle rendite che la Società ritrae dalle entrate, dalle vendite di animali doppi ecc. si argomenta il vistoso introito annuale.

Suoi organi sono: i *Proceedings* e le *Transactions of the Zoological Society of London,* ove comparvero lavori di grande mole e d'immenso interesse scientifico e la cui Raccolta completa costituisce una delle opere più costose e più desiderate da qualunque zoologo.

Do qui la lista degli Animali più rari che osservai nei Giardini:

MAMMIFERI.

Cercopithecus thoesti — nuova Scimmia dell'Africa.

Pithecia chiropotes — rara Scimmia Americana.

Rhinoceros lasiotis — l'esemplare tipico ricevuto nel febbraio 1872.

Tragelaphus selousi — Antilope del Lago Ngami, uno dei tipi della nuova specie.

Strepsiceros imberbis — giovane maschio ricevuto di recente dal Somaliland.

2 *Babirussa alfurus* — Suino ungulato di Celebes.

UCCELLI.

2 *Psephotus chrysopterygius* — Pappagalli Australiani, i due soli ritenuti viventi in Europa.

Chunga burmeisteri — Cariama dell' Argentina settentrionale.

Aptenodytes forsteri — il Re dei Pinguini.

Apteryx haasti — della Nuova Zelanda.

Varii individui delle due o tre forme di Struzzo, ora generalmente riconosciute, cioè *Struthio molybdophanes* del Somaliland e *S. australis* del Capo di Buona Speranza.

Casuarius philipi — esemplare tipico di un nuovo Casuario della Nuova Guinea.

RETTILI, BATRACI E PESCI.

Macroclemmys Temmincki — bell'esemplare del Sud degli Stati Uniti d'America.

Python reticulatus — un immenso esemplare di Pitone reticolato del Malacca di 6 metri di lunghezza.

Dasypeltis scabra — varii esemplari dall' Africa.

Naia bungarus — parecchi esemplari di questo serpente molto velenoso.

Amphimba means — Batracio dell'America del Nord.

Ceratodus forsteri — due esemplari di questo raro animale portato da Mr. O'Connor dal Queensland (1).

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(1) P. L. Sclater, List of some of the more important animals of scientific interest now living in the Society's Gardens (1898).

Altra cosa notevole che vi ammirai fu l'Esposizione di gigantesche Tartarughe viventi prestate per l' occasione dall'on. Walter Rothschild.

Durante il periodo Pliocenico od in una divisione posteriore del periodo Terziario le Tartarughe gigantesche erano largamente distribuite nel mondo, come è attestato dai resti fossili che vennero trovati nell' India, nell'America del Sud e del Nord ecc., e la più grande di esse fu la nota *Testudo atlas* delle Colline Suvalik dell'India settentrionale, la cui lunghezza del carapace era di sei piedi, circa m. 1.80. Ma al presente periodo storico esse scomparvero per non vivere che in poche isole che costituiscono tre gruppi distinti. Due di questi sono situati nell'Oceano Indiano e comprendono il Gruppo Aldabra al Nord-Est di Madagascar, ed il Gruppo delle Mascarene — Riunione, Mauritius e Rodriguez — situate all'Est della stessa grande isola; mentre l'ultimo è quello della Galopagos. Il numero totale delle specie di Tartarughe viventi, che si sa aver esistito nel periodo storico, è di circa quattordici, caratterizzate dalla loro grande statura, dai loro lunghi colli, e dai carapaci bruno-uniformi o neri. Esse si dividono in tre Sezioni, secondo la loro distribuzione geografica, ognuna delle quali va distinta dalle altre per certe particolarità di struttura. Questo splendido gruppo composto di 65 esemplari, affatto unico e che probabilmente non si potrà più vedere, fu formato dall'on. Walter Rothschild, esso rappresenta sei specie, delle quali do qualche dettaglio:

Sez. *A*. Tartarughe del Gruppo Aldabra

1. *Testudo Daudini* (Tartaruga dell' Aldabra meridionale). Quattro esemplari colle seguenti dimensioni:

	N.° 1	N.° 2	N.° 3	N.° 4
Lunghezza sopra il carapace	mm. 1100	675	600	1675
Lunghezza in linea diritta	„ 950	525	475	1350
Larghezza sopra il carapace	„ 1075	675	600	1675

2. *Testudo elephantina* (Tartaruga dell'Aldabra settentrionale). Cinque esemplari, dò le dimensioni di tre di essi:

	N.° 1	N.° 2	N.° 3
Lunghezza sopra il carapace	mm. 1125	525	455
Lunghezza in linea diritta	„ 1025	410	350
Larghezza sopra il carapace	„ 1250	455	475

3. *Testudo gigantea* (Tartaruga dell'Isola Charles). Esemplare femmina delle seguenti dimensioni:

Lunghezza sopra il carapace	mm.	1025
Lunghezza in linea diritta	„	700
Larghezza sopra il carapace	„	1125

Sez. *B*. Tartarughe delle Mascarene.

4. *Testudo inepta* (Tartaruga ruvida). Un grande maschio delle seguenti dimensioni:

Lunghezza sopra il carapace	mm.	1225
Lunghezza in linea diritta	„	950
Larghezza sopra il carapace	„	1275

Sez. *C*. Tartarughe delle Galopagos.

5. *Testudo vicina* (Tartaruga dell' Albermarle meridionale). Trentacinque esemplari, e qui presento le dimensioni dei due più grandi e di quello più piccolo:

	massimo	2° massimo	minimo
Lunghezza sopra il carapace	mm. 850	825	225
Lunghezza in linea diritta	„ 650	625	175
Larghezza sopra il carapace	„ 875	885	225

6. *Testudo ephippium* (Tartaruga dell'Isola Duncan). Diciannove esemplari, ecco le dimensioni dei due più grandi e del più piccolo:

	massimo	2° massimo	minimo
Lunghezza sopra il carapace	mm. 900	875	550
Lunghezza in linea diritta	„ 800	775	475
Larghezza sopra il carapace	„ 900	875	550 (1).

Questo stesso giorno ebbi la ventura di fare la personale conoscenza del prof. Antonio Reichenow, Direttore della Sezione Ornitologica del Museo di Berlino e della graziosa sua Signora e

(1) P. L. Sclater, Exhibition of Gigantic Tortoises, Zoological Society of London (1898).

serbo il più gradito ricordo di tante ore passate in seguito con Loro.

Nella serata alle ore 21 ebbe luogo il grande ricevimento con suntuoso buffet offerto da Sir John Lubbock, presidente del Congresso, al Museo di Storia Naturale, Cromwell Road. I naturalisti vi accorsero numerosi ricevuti con fasto ed amabilità da Sir Lubbock e dalla gentile sua Signora. Fu in questa memorabile sera che noi mettevamo piede nel più ricco Museo del mondo dal lussuoso Edificio, dalle Raccolte straordinariamente ricche ed impossibili ad eguagliarsi. Non era aperta che la *Central Hall* e i due rami laterali al piano superiore, ove da un lato sono conservate alcune Antilopi e dal lato opposto le Raccolte di Uccelli mosca di John Gould, che gli servirono di materiale pella grande sua opera sui Trochilidi. A dare una semplice idea della grandiosità dell' edificio basterà il ricordare le dimensioni della *Central Hall* lunga 170 piedi (circa m. 52), larga 97 piedi (circa m. 30) e alta piedi 72 (circa m. 22).

Dall'amico Brusina fui presentato al Bowdler Sharpe, Assistant-Keeper Sub-department Vertebrata of the B. M., forse il più grande degli Ornitologi viventi od almeno fra i più illustri e stimati. Io non posso con adeguate parole esprimere quanto affabile e buono mi si dimostrò accondiscendendo a permettermi di fare a tutto mio agio studi ed esami sopra esemplari della Raccolta Ornitologica e quanto fu indulgente onorandomi tosto della sua amicizia. Fosse pure per questa sola auspicatissima conoscenza, la serata del 27 agosto 1898 formerà epoca indimenticabile nella mia vita.

Domenica 28 agosto. Abbenchè nel continente sia noto che di domenica tutto tace e riposa nella bella Terra di Albione, pure nessuno può immaginare a quale eccesso sia portato quest'uso. Le botteghe chiuse, le vie deserte, perfino la posta non parte e può dirsi che Londra si trasforma in una città d'infimo ordine. Tutti vanno in campagna a far gite, escursioni e per ventiquattr'ore quest' immensa lotta per l'esistenza è sopita.

Alle 2.30 pom. ci venne offerto un ricevimento con rinfresco allo stesso Museo di Storia Naturale in Cromwell Road ed accordata la libertà di vedere le grandiose collezioni.

Facevano questa volta gli onori di casa i signori R. B. Sharpe, H. Woodward e W. G. Ridewood. Il *British Museum* data dal 1753 e fino al 1860 conteneva ogni sorta di Raccolte, quando,

sopra proposta del primo Lord della Tesoreria, quelle di Storia Naturale si decise venissero separate e collocate in apposito palazzo, il quale venne cominciato nel 1873 sui piani del capitano Fowke e del sig. Waterhouse e venne aperto al pubblico in sulla fine del 1881 ; il contratto primitivo fatto dal Governo coi sig.i George Baker and Sons of Lambeth fu per la somma di Sterline 352 mila, pari a nove milioni di franchi, ma poi, per nuove aggiunte, essa dovette aumentarsi. Il Museo è il più grande, se non il solo fabbricato moderno (1), nel quale la terra cotta fu adoperata ovunque dalla facciata esterna alla superficie dei muri interni, comprendendo altresì tutte le varie decorazioni e gli ornamenti.

L'area del fabbricato, compresa la parte staccata ove sono conservati gli animali in alcool, supera di poco un Ettaro e mezzo di superficie e col totale dello spazio circostante occupato dai giardini, che si stendono dai lati sud, est ed ovest, è di poco inferiore agli Ettari 5 $^{1}/_{2}$.

Parlare delle varie Raccolte eccezionalmente ricche di zoologia, geologia e paleontologia sarebbe oltrechè ozioso, non serio in questo succinto resoconto. Figurarsi che la sola Raccolta ornitologica comprende il numero stragrande di 400 mila esemplari! Del resto la Direzione del *British Museum of Natural History* con lodevole pensiero sta pubblicando in una serie di grossi volumi la illustrazione completa delle sue Raccolte, affidandone le varie monografie agli specialisti più insigni. La serie Ornitologica, comprendente ventisette grossi volumi con circa 400 tavole colorate e 23 mila pagine di stampa, è quasi del tutto illustrata, cinque di essi sono già esauriti e la loro serie completa costituisce una delle copie librarie più rare, più costose e più utili. Per noi Italiani è una vera compiacenza che tre dei volumi più importanti siano stesi dall' illustre ornitologo connazionale il conte prof. Tommaso Salvadori di Torino. Il Sharpe ci volle condurre nel suo gabinetto di lavoro, dal quale tante opere grandiose furono lanciate nel mondo scientifico e là ci narrò a grandi tratti la storia di questo Museo di Storia Naturale, delle sue vicende e delle sue ricchezze e vi potemmo ammirare una copiosa collezione di fotografie degli ornitologi di ogni paese.

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(1) General Guide to the British Museum Nat. Hist. Cromwell Road, printed by Order of the Trustees (1896).

Alla sera sulle 9 pom. il Presidente del Consiglio del Royal Societies Club dava un ricevimento ai Zoologi nel suntuoso palazzo di St. James' Street a cui intervenivano soltanto gli uomini.

Lunedì 29 agosto. Tale giornata era devoluta alla visita del grandioso Museo Zoologico dell'on. Walter Rothschild a Tring, che trovasi nell'Herts a circa due ore da Londra. Noi partimmo da Euston Station alle ore 9.50 ant. con uno splendido treno di Wagon-salons messo a nostra disposizione dall' on. Lord. Alla stazione di Tring, cui giungemmo direttamente dopo una rapidissima corsa, ci attendeva col sig. Ernesto Hartert, distinto ornitologo e direttore del Museo di Tring, lo stesso on. Walter Rothschild colle sue carrozze per condurci alla Villa che dista dalla Stazione circa un'ora. Attraversammo un bellissimo parco variato per leggere ondulazioni di terreno, per collinette e per boschetti che si succedevano. La tenuta di Tring è anche una buona riserva di caccia, ove il Rothschild fa ogni anno grandi partite con numerosi inviti. Abbondantissime vi scorgevamo le pernici o vagare nei campi del frumento mietuto o in grossi branchi levantesi a volo impaurite dal nostro passaggio. Il Museo, come leggesi nella Guida, di cui fu donata una copia a tutti gli intervenuti (eravamo in centosessanta, fra cui parecchie signore), ebbe principio con una piccola raccolta di Farfalle e di Uccelli fatta dall' on. Walter quando era fanciullo; poi gradualmente queste collezioni assunsero tali proporzioni ch' Egli ideò di costruire un'apposita fabbrica ad uso di Museo. È diviso in due sezioni, l'una che raccoglie animali di ogni classe ed è aperta al pubblico a dati giorni, l'altra situata ai lati delle gallerie pubbliche e racchiude soltanto oggetti di Ornitologia e di Entomologia ed una ricchissima biblioteca zoologica e che rimane riservata soltanto agli studiosi per le ricerche scientifiche. Attualmente coadiuvano l'on. W. Rothschild il dott. Ernesto Hartert e pella sezione Entomologica il dott. Carlo Jordan oltre a varî Imbalsamatori ecc. A cura poi del Rothschild e dei suoi valenti collaboratori viene pubblicato tutti gli anni un grosso volume con parecchie tavole ove sono resi di pubblica ragione i risultati e le ricerche scientifiche fatte nel Museo. Il fabbricato fu appositamente eretto ad uso di Museo con tutte le moderne comodità. La grande sala ove sonvi le Raccolte di Mammiferi, di Uccelli, Rettili ecc., spazia dall'alto al basso del fabbricato e si accede ai varii suoi piani con comodi e larghi pianerottoli, le cui

scale rimangono al di fuori della stessa sala, il tutto illuminato a luce elettrica.

Immensi i tesori ammassati; per dare una languida idea della sola Raccolta Ornitologica dirò che comprende circa 150 mila esemplari, che vi si ammirano le più grandi rarità, come ad es. la *Pennula ecaudata*, un Voltolino ora estinto delle Isole Sandwich, un maschio adulto ed uno giovane del *Camptolaimus labradorius*, anitra estinta del Nord America, una splendida *Alca impennis*, altra specie ora estinta e via via. Il Rothschild avendo poi una spiegata predilezione pelle aberrazioni di colorito e sugli Ibridi ne ha messo assieme una preziosa serie. Davanti a tanta grandiosità tutti rimangono colpiti di stupore, ma il Naturalista impreca alla mancanza del tempo che gli impedisce di esaminare e di apprezzare i tesori. Fu l'impressione che provammo concordi nel Museo Rothschild. Scesi nel parco abbiamo vedute parecchie specie di Canguri, di *Apterix* e di *Casuarius* che vi si custodiscono e fra queste alcune per la prima volta portate viventi in Europa. Ci venne quindi servito un suntuoso banchetto nella gran sala dell'Agenzia ed ai brindisi dei Sig.i Jentink e Vaillant rispose con le più lusinghiere espressioni l'ospite generoso.

Infine furono fatte alcune fotografie di vari gruppi di Naturalisti, carissimo ricordo della stupenda gita a Tring.

L' on. Walter Rothschild che è il figlio primogenito del primo Barone e Lord Rothschild, nacque nel 1868 e fu educato a Bonn e poi a Cambridge al Magdalene College. Ha già assicurata la fama di grande Naturalista, nè v'ha dubbio che a Tring Park possiede la più bella raccolta zoologica privata del mondo, dicesi vi spenda più di mezzo milione all' anno. Recentemente egli fu eletto membro del Parlamento per Aylesbury nella divisione del Buckinghamshire.

Martedì 29 agosto. Per oggi S. G. il Duca di Bedford aveva invitato sessanta Naturalisti a visitare le Raccolte di Cervi viventi che tiene al suo Castello di Woburn. Vi andammo egualmente partendo da Euston-Station, attesi alla piccola stazione di Richmond dalle carrozze del Duca e ricevuti dal sig. R. Lydekker, sempre compito e gentile, che ne giustificò l'assenza; vedemmo le ricche serie dei magnifici Cervi passeggiando a lungo nel vasto parco ciò che rese doppiamente gradita la suntuosa colazione che ci fu servita.

Reduci a Londra nella serata il sig. R. Bowdler Sharpe ci

offrì un pranzo al Museo di Storia Naturale. Gli invitati erano: col sig. R. B. Sharpe, la moglie e la figlia miss Emily, il prof. Brusina, il prof. Stejneger di Washington, il prof. Jentinck di Leida, il prof. R. Blanchard di Parigi con moglie e figlia, il sig. Oates di Londra, il prof. Reichenow di Berlino con la sua signora, il sig. Bemmelen di s' Gravenhage ed il sottoscritto. Vi regnò la migliore allegria, vivaci i molti brindisi cui lo Sharpe rispose colla più calorosa espansione. Dopo il pranzo colla ferrovia sotterranea il sig. Sharpe volle condurci all' *Exhibition,* ove al Teatro davano la riproduzione grandiosa della battaglia di Santiago di Cuba.

Da un grande anfiteatro, ove erano addensate più migliaja di persone si prospettava un gran lago che figurava il bacino del porto; erano riprodotte la città, il forte Morro, le batterie e la fortezza Spagnuola. Vi comparve la flotta Spagnuola fuggente, inseguita dalla poderosa Americana, il cannoneggiamento successe insistente e continuato, le torpedini esplodevano, le batterie di terra tentavano respingere ed infine la flotta spagnuola fu incendiata, sommersa e distrutta. Uno spettacolo di effetto mirabile e così ben eseguito che l'illusione si poteva dire perfetta. Poi girammo per l'Esposizione, ove v'erano i più svariati spettacoli ed infine colla stessa ferrovia sotterranea ritornammo a casa.

Chi non l'ha percorsa questa fantastica ferrovia non può farsene idea. Entrati in un ufficio qualunque, discendete per un via vai di corridoi sino a che vi trovate alla stazione con tutti i suoi binari, le sue piattaforme, i treni che vanno e vengono con la nota celerità e fra tanta ressa di passeggieri una calma ed un ordine perfetto. Vi trovate in un mondo tutt'affatto nuovo, siete desto o sognate? Non vi rimane che una sola risposta — siete sotterra!

Martedì, mercoledì e giovedì 30, 31 agosto e 1 settembre vi furono assaggi con scandagli a Plymouth col Direttore del Laboratorio Biologico marino ed a Port Erin, Isle of Man, sotto la direzione del prof. Herdmann, F. R. S. Non potei raccogliere ragguaglio intorno a queste gite, ma se non sbaglio esse non ebbero luogo per lo scarso numero degli aderenti. A Londra ci fu sempre mantenuto libero l'ingresso ai Giardini della Società Zoologica, agli appartamenti della Società Linneiana, al Club della Società Reale ed altri Corpi ed Istituti scientifici. Dappertutto fummo colmati di molte amabilità.

È così il Congresso è finito.

Il Brusina ed io ci fermammo ancora parecchio a Londra per visitare a tutto agio il Museo di Storia Naturale ed i Giardini della Società Zoologica colla valida ed amica guida dello Sharpe, del Woodward e dello Sclater.

Mercoledì 1 settembre. Accettammo l' invito fattoci a Cambridge dal Rev. Canonico A. M. Norman, notissimo naturalista, e ci recammo a visitare la sua ricchissima Collezione di animali marini invertebrati a Berkhamsted nell' Herts. Il prof. Brusina, intelligentissimo in materia, rimase colpito della grandiosa Raccolta e della non meno ricca biblioteca. Partiti da Londra da Euston Station alle 9 del mattino, vi facevamo ritorno alle 5 di sera dopo di aver passato una lieta giornata nella cortese ospitalità del Norman.

Venerdì 3 settembre. L'ottimo nostro amico R. B. Sharpe ci aveva invitati a visitare secolui il castello reale di Windsor. Si associarono alla gita la colta ed amabile sua figliola Miss Emily, nota specialmente pegli eccellenti lavori sulle Farfalle del Sud-Africa e il prof. Reichenow di Berlino colla gentile sua Signora, figlia al grande ornitologo Cabanis. Partiti da Paddington Station in breve fummo a Windsor, ove potemmo visitare il grandioso castello reale, la Tomba del Principe Consorte e tante altre cose notevoli; indi ripreso il treno smontammo a Staines, ove lo Sharpe, sempre previdente, sempre liberale, ci aveva fatto imbandire il pranzo in una stupenda posizione sul Tamigi, là continuamente percorso da innumerevoli barchette della Società dei Canottieri nei loro brillanti costumi. Giovani d'ambo i sessi lo solcavano in ogni direzione girando fra i Cigni maestosamente natanti in tanta gaiezza di vita. La sera tornammo grati al sig. Sharpe ed alla sua degna figliuola di aver saputo con tanta industre gentilezza renderci maggiormente gradito il soggiorno di Londra.

Venuti a Parigi vi potemmo ammirare comodamente le grandiose Collezioni del Museo di Storia Naturale, quella di Uccelli mosca, ragni e scorpioni del notissimo aracnologo E. Simon, la Ornitologica di J. Vian, venerando scienziato francese, la conchiologica del Dautzenberg che col Barone di Guerne e col Simon ci usarono molte amabilità.

Quanti altri nomi di illustri scienziati dovrei qui aggiungere o perchè nuovi trovati mi procurarono l'onore di conoscerli o perchè relazioni contratte mi confermarono la loro deferenza, ma il carattere di questo modesto resoconto, di questo semplice diario

impostomi dai Corpi che ebbi il pregio di rappresentare [1] non me lo concede. Mi sarà permesso però un'eccezione ai riguardi del sesso gentile.

E le signore Hubrecht, Haeckel, Sharpe, Delage, Dollfus, Blanchard, Reichenow, Hartert, Lubbock, Simon, le signorine E. Sharpe, Vaillant e Blanchard serie nelle discussioni scientifiche, amabili nei famigliari convegni sento di ricordarle e sotto il loro simpatico patronato chiudo il disadorno lavoro.

Padova, 9 maggio 1899.

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(1) Essi erano il R. Istituto Veneto, l'Accademia di Verona, quella di Acireale, la Società Italiana di Sc. Nat. di Milano, la Veneto-Trentina di Padova cui s'aggiunga la Rivista Ital. di Sc. Nat. di Siena.

(Finita di stampare il giorno 11 settembre 1899)

24. Notizie sulle peregrinazioni autunnali del *Garrulus glandarius* nella Provincia di Padova ad un quinquennio di osservazioni. *Ibid.* f. 6. Siena, 1890.
25. Studi sugli uccelli uropterofasciati, con tre tav. col. *Atti Soc. Ven. Tr.* Vol. XI, fasc. II. Padova, 1890.
26. Sopra un individuo di *Querquedula crecca* anormalmente colorito. *Atti Soc. Ven. Tr. Sc. Nat.* Vol. XII, fasc. I, con tav. col. Padova, 1891.
27. La Caccia in Valle. Brano per nozze Lonigo-De Zigno. Padova, 1891.
28. I Cigni nel Veneziano. *Gazzetta di Venezia*, due numeri del febbraio 1892.
29. Cattura di uccelli negli anni 1890-91. *Riv. Ital. Sc. Nat.* XII. Siena, 1892.
30. Cenni sulla Raccolta Ornitologica del R. Istituto Tecnico di Bergamo. Aberrazioni nel colorito del piumaggio. I, Uccelli Italiani. *Atti Soc. Ven. Tr.* Vol. I, ser. II, fasc. I. Padova, 1892.
31. Su di un *Monachus atricapillus* a becco anormale, con tav. *Ibid.* Padova, 1892.
32. Le *Branta leucopsis* nel Veneto. *Atti Soc. Ital. Sc. Nat.* Vol. XXXIV, fasc. 1. Milano, 1893.
33. La *Fuligula Homeyeri,* Baed. nel Veneto. *Ibid.* fasc. 2. Milano, 1893.
34. Notizie su di un ibrido *Lagopus mutus* e *Bonasa betulina* della Raccolta Camozzi-Vertova, con tav. col. *Ibid.* fasc. 3. Milano, 1893.
35. Catture di uccelli avvenute durante l'anno 1892. *Riv. Ital. Sc. Nat.* XIII. Siena, 1893.
36. Su di un ibrido *Mareca penelope* e *Anas boscas* preso nel Veneto. *Atti Soc. Ven. Tr. Sc. Nat.* Ser. II, vol. II, fasc. 2. Padova, 1893.
37. Anomalie nel colorito del piumaggio osservate in 216 individui della mia Collezione Ornitologica Italiana. *Atti Soc. Ital. Sc. Nat.* Vol. XXXIV, fasc. 2. Milano, 1893.
38. Il *Turdus fuscatus* nel Bergamasco. *Riv. Ital. Sc. Nat.* XIII, n. 9. Siena, 1893.
39. Note ornitologiche. *Riv. Ital. Sc. Nat.* XIX, II. Siena, 1894.
40. La *Tadorna cornuta* nel Veneziano. *Lo Sport illustrato.* XIII, n. 595, pag. 41. Milano, 1894.
41. L'*Haliaetus albicilla* nel Veneziano. *Ibid.* N. 598, pag. 77. Milano, 1894.
42. Materiali per la Fauna Padovana dei Vertebrati. I (Mammiferi, Rettili, Anfibi e Pesci). *Atti Soc. Ven. Tr. Sc. Nat.* Ser. II, vol. II, fasc. 1. Padova, 1894.
43. *Idem.* II, Uccelli. *Atti Soc. Ital. Sc. Nat.* Vol. XXXIV. Milano, 1894.
44. Die Ornithologische Litteratur Italiens vährend der Jahre 1891 bis 1893. *Journal für Ornith.* XLII. Berlin, 1894 (in collaborazione col dott. P. Leverkühn).
46. La Caccia di Botte o di Valle nella Laguna di Venezia, con fig. *Lo Sport illustrato,* varii numeri. Milano, 1894.
45. Sopra cinque ibridi selvatici del genere *Fringilla* presi in Italia. *Ibid.* Vol. XXXV. Milano, 1895.
47. Le ultime apparizioni dell' *Actochelidon sandvicensis* nel Veneziano. *Atti Soc. Ital. Sc. Nat.* Vol. XXXVI. Milano, 1896.

48. Sopra una *Querquedula crecca* anormalmente colorita. *Riv. Ital. Sc. Nat.* XVI, n. 10. Siena, 1896.
49. Note ornitologiche. *Ibid.* N. 11. Siena, 1896.
50. Sopra una varietà di *Nyroca africana*, con tav. *Atti Soc. Ital. Sc. Nat.* Vol. XXXVI. Milano, 1897.
51. La recente cattura di un Fenicottero nel Veneziano. *Ibid.* Milano. 1897.
52. Anomalia di colorito nella *Q. crecca*, con tav. *Ibid.* Milano, 1897.
53. Note ornitologiche per l' anno 1895. *Ibid.* Milano, 1897.
54. Un *Gennaja Feldeggi* colto in Calabria. *Avicula.* I, fasc. 6. Siena. 1897.
55. Recensione dell' opera " Gli ibridi naturali tra gli uccelli " di A. Suchetet. *Ibid.* Siena, 1897.
56. Sopra gli ibridi del tipo *Chaulelasmus streperus* ed *Anas boscas* colti in Italia. *Atti R. Ist. Ven. sc. lett. ed arti.* Tom. VIII, ser. VII. Venezia, 1897.
57. Notes on some Specimens of *Anatidae* in the late Count Ninni 's Collection. In *The Ibis* for January 1898. London, 1898.
58. Una varietà di colorito osservata in un' *Anas boscas. Atti Soc. Ital. Sc. Nat.* Vol. XXXVII. Milano, 1898.
59. Le recenti comparse del *Puffinus Kuhli* nel Veneziano. *Ibid.* Milano, 1898.
60. Letter on the *Anser neglectus,* Sushkin, addressed to the Editors of the Ibis. In *The Ibis* for January 1898. London, 1898.
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62. La nidificazione del *Milvus migrans* nel territorio Veronese. *Atti R. Ist. Ven. sc. lett. ed arti.* Tom. IX, ser. VII. Venezia, 1898.
63. Eine Brutstätte des schwarzen Milans bei Grezzano bei Verona. *Journ. für Ornith.* Oktoberheft 1898. Berlin, 1898 (traduzione di O. Haase).
64. Ornithological Notes on thirty abnormal coloured *Anatidae* caught in the Venetian territory. *Ornis,* IX, fasc. 2°. Paris, 1898.
65. The copious apparition of the Buffon 's Skuas on the Lake of Garda in September 1898. In *The Ibis* for January 1899. London, 1899.
66. Elenco degli uccelli rari o più difficili ad aversi conservati nella sua Collezione Ornitologica Italiana al 31 dicembre 1899. *Ornis,* IX, fasc. 3°. Paris, 1899.
67. Materiali per una Fauna Ornitologica Veronese, con note di Vittorio Dal Nero. *Atti R. Ist. Ven. sc. lett. ed arti.* Tomo LVIII, parte II, e *Atti Soc. Ital. Sc. Nat.* Vol. XXXVIII. 1899.
68. The Nesting of the Black Kite in the territory of Verona. In *The Zoologist* for June 1899. London, 1899.
69. Notizie sulle comparse recenti del *Syrrhaptes paradoxus* in Austria ed Inghilterra. *Avicula.* III. Siena, 1899.
70. L' *Aquila rapax* (Temm.) ed il *Buteo desertorum* (Daudin) per la prima volta osservati in Italia *(in corso di pubblicazione).*

With the Author's Compliments 13

RELIGIO OBSTETRICI

A FAREWELL ADDRESS TO THE GRADUATES OF THE UNIVERSITY OF EDINBURGH ON JULY 28TH, 1905

BY

PROFESSOR A. R. SIMPSON, M.D., D.Sc.

Reprinted from *The Scottish Medical and Surgical Journal*, August 1905.

RELIGIO OBSTETRICI

A Farewell Address to the Graduates of the University of Edinburgh, on July 28, 1905, by Professor A. R. SIMPSON, M.D., D.Sc.

LADIES AND GENTLEMEN, MY FELLOW-GRADUATES,

I bid you welcome to the ranks of an honourable profession. As I have had the privilege of presenting you for promotion to the Vice-Chancellor, it falls to me on behalf of my colleagues and myself to offer you our warm congratulations on the possession of degrees which you have earned by your intelligence and industry, and to bid you God-speed as you enter on a new phase of your career.

ANTICIPATIONS.

When this ceremony was in process a year ago, various thoughts passed through my mind. Looking forward to to-day, I thought if I still had a place on this platform it would be my lot to serve for a third time as Promoter of the graduates. Further, when the Bachelors of Medicine were entering their first graduation year, the promoter would be entering the year of his graduation jubilee; seven times seven summers having passèd over my head since, with eight and sixty other men—"sweet girl graduates in their golden hair" were as yet but shadows in a poet's dream—I passed before the venerable Principal Lee to be capped, and to hear him say, *Te medicinæ doctorem creo.* Again, I kept thinking that the Colleague who was the promoter of the day would be an ideal Dean for the Faculty of Medicine, and that it would be for the interests of the University that I should let the office pass to the keeping of Professor Cunningham. Moreover, having heard from time to time as I passed along the Infirmary corridors a sound as of the grinding of an axe that was to be laid at the root of certain trees, I judged that the tree that counted seventy rings beneath

its bark would be the first to feel its edge. The woodman, meantime, spared that tree. Still, I recognise that they who would set an age-limit to the tenure of such a Chair as mine are amply justified. There is always risk that the ageing incumbent of a Chair may become an incubus upon it. I could not bear to dree that weird. Accordingly, I have carried out another thought that was in my mind. I have resigned to the charge of the University Court the commission with which the Curators entrusted me on the 4th of July 1870. For, though I be not breathless yet, I judge that after a course of five-and-thirty years, it is fitting I should hand on the torch of Midwifery in the University to an after-runner, who may carry it over with brighter glow to another generation.

RESIGNATION.

It is not without a pang that I pass it on. Whether we have regard to principles or practice, my department of medicine yearly grows more intensely interesting to the scientific explorer, more urgently important for the general practitioner. Problems that have wrinkled the brows of the wisest through half a millennium are finding their solution now: operations are daily faced with confidence to-day, from which half a century ago the boldest held aloof.

Progress becomes more and more rapid because of the increasing facility of international commerce. The great community of the devotees of Lucina knows no limit of race or tongue, and what is discovered anywhere is swiftly made known everywhere. Hence a multiplication of periodicals that beats one to keep up with. Not to speak of writings in our own language, when I began to lecture there was one obstetric journal recently started in German. For some years I have taken in four, besides looking at two others in the library, and seeing special papers in general medical magazines. In French, in 1870, I knew of no special journal in Midwifery: now there are at least four from Paris, besides bulletins of two societies. For some time I have had three Italian journals coming to me, besides that I see another in the library. A society in Bucharest sends me its transactions, written not in Roumanian, happily, but in French; and even whilst Russia and Japan are in deadly

conflict, our Russian and Japanese brethren continue to make contributions to our literature. I refer to these accumulating publications as an evidence to you of the rapidly growing extent and interest of the themes that have to be dealt with from a Chair of Midwifery, and as an excuse to myself for wishing to see this—the oldest Chair of Midwifery in the world—freshened with the energy of a younger occupant.

The enormous and rapidly extending progress which it has been my delight and my duty to follow out in one department of the healing art is in line with what has taken place in all the subjects embraced in your curriculum, and has been attended with changes in the training of entrants on the medical profession, to two of which I shall advert.

A MEDICAL APPRENTICE.

When I began to study, the system of medical apprenticeship had not quite died out. Some of my fellow-students were apprenticed to Professor Spence, who was at that time an extra-mural Lecturer on Surgery. I had myself the singular good fortune to be apprenticed to Professor Goodsir. The relationship did not, I confess, help me much in learning to cure disease. He sometimes had me with him in a curiously constructed two-wheel carriage of his own designing, something like a hansom, hung low on the axle, and turned the other way, so that you entered from behind, and sat looking backward. But the only patient whose case remains in my memory was that of a lady with chronic ulcers in the leg, that he set me to dress daily with Red Wash. Healing was going on but slowly till, with the advice of my uncle's then assistant, Dr Drummond, I changed the Red for Black Wash ; and when it occurred to my master to enquire, after some weeks, how the sores were getting on, I could report them nearly well. But the apprenticeship did me this good service, that it gave me opportunities for much dissection, and so for acquiring a facility in the handling of forceps and scalpel that has been a life-long benefit. What was of still greater moment, it brought me under the spell of a man of genius, who was a tireless toiler, and who sought in all his own researches and the work of his assistants what he called "God's truth." When I bade him good-bye, on taking my

departure for the Continent after graduation, he gave me introductions to Johannes Müller and Professor Arnold, and some hints as to the best use of my time. He ended by saying in his slow, emphatic way, "There's another thing, Mr Simpson. See that you go regularly to church every Sunday." I think my eyes must have asked him if he feared I might be a scapegrace, for he added with a smile, "There's no better way to learn German."

I do not know of any survivor among those of my fellow-students who were apprenticed to Professor Spence, and I expect that the last of the Edinburgh medical apprentices will be buried in my grave.

EXIT APPRENTICESHIP: ENTER EXAMINATIONS.

You know by distressful experience—and so do your examiners—how the examination system has grown up among us till it has become a multiplied misery. So much so that when, some years ago, I wrote to the wife of our late M.P. that a sixth son had been born to me, Lady Priestley replied, "Six sons. To think what that means in examinations!" This increase in examinations has become a necessity, very much because of the extension of all the subjects of study and the attendant specialisation and development of new departments. When you were attending the Arts Classes or preparing for your Preliminary Examination, you witnessed the genesis of a new Chair by a process of fission that took place in the organism of Medical Jurisprudence and Public Health. You may, without great stretch of imagination, conceive it possible that Sir Henry Littlejohn's predecessor might on occasion lecture for the Professor of Materia Medica, when you are reminded that Sir Douglas Maclagan was promoted from the extra-mural Lectureship on Materia Medica to the Chair he so long adorned, and that during most of his professoriate he served with four of his colleagues as an Infirmary Physician and Professor of Clinical Medicine. But will you believe me when I tell you that I have heard his predecessor, Professor Traill, lecture on Natural History—a subject that since his day has also fissioned into the two separate Chairs of Zoology and Geology? There were giants in those days. But I leave you to judge how far it was due to the wide range of their capacities,

or to the narrow range of the medical disciplines, that it was possible for one professor thus to take another's place. Professors were sometimes translated from one Chair to another. When Professor Alison resigned the Chair of Practice of Physic in 1855, Sir James Simpson was urged from various sides to become a candidate. I heard a leading member of the Town Council—one who had signed his commission to the Chair of Midwifery in 1840—assure him that he would be elected if he stood for Alison's Chair. The coveted seat was won by a general practitioner from York. On the afternoon of his election, Professor Laycock took from his vest pocket his silver lancet-case and made me a present of it, saying he would now have no further use for it. Dr Alexander Wood, who was second in the running, and only lost the Chair by a single vote, had no place in the Infirmary. He was one of the Maternity Physicians, and was so vexed with Sir James for supporting the claims of Professor Bennett and Dr Laycock in preference to his own, that he spoke of starting an opposition class of Midwifery. You needn't be surprised, therefore, that Professor Traill could undertake a week's work in the Natural History class-room, for that versatile gentleman was reputed to be prepared at short notice to take the place of any of his colleagues who might be ill or absent—only drawing the line at the blood-shedding class of Clinical Surgery.

ARCHAIC EXAMINATIONS.

Hence you will the more easily comprehend how it was possible for me to reach the stage in my medical career at which you have this day happily arrived, with no severer strain than was implied in two written examinations of two days each —one at the end of my third, and the other at the end of my fourth and final session. There was, indeed, an oral examination administered to four of us on the second occasion. But it was of the simplest. Of the tables placed in the Court-Room for the six subjects of the final examination, I was first set down before that of the Professor of Pathology. Professor Henderson told me that two of the questions had been answered well enough, but that the third was all wrong. When a word or two showed him that I had misunderstood the

question, he said that several other candidates had made the same mistake and passed me on to Professor Traill. That admirable gentleman told me that he would have put a higher value on my exercise if, like my companions, William Playfair and John Somerville, who were being interviewed at the same time at other tables, I had taken pains to copy on the margin a diagram, with which his lectures had been illustrated, showing a cross section of the *cloaca maxima* in Rome. So they let me go. But I had almost forgotten my preliminary examination. It was in Latin: my examiner, Professor Syme. In the Latin class-room he put a copy of Virgil in my hand, and when I had read and translated a dozen lines in the Æneid he was satisfied. So was I. That he was not a very hard examiner you may guess when you hear that he passed another lad from the country who fairly broke down and began to shed tears, as he was trying to toil through the lines that tell how Achilles in his chariot dragged the dead body of Hector by the heels round the walls of Troy. Syme encouraged him to go on, saying, "But why should you cry?" The candidate answered, "Oh Sir, I'm wae for Hector."

THE GRADUATE'S HORIZON.

When you compare your own experiences with these which must appear to you primeval, you will begin to see that stupendous changes have swept over all the fields of Medicine during the intervening half century. You will know that in the pursuit of your profession your mind will be kept engaged with themes of perennial interest, and your present attainment will be an incentive to further effort. And, because your diploma bears the honoured seal of a great university, you will never forget that the range of your interest is not confined even within the expansive bounds of Medicine, but has a horizon co-extensive with the Universe.

"YET ONCE"—"YET ONCE MORE."

Now, the era I have spanned has been one of those "once more" epochs which the Hebrew mind has taught us to discern, when we see the Supreme Power "shake the heavens, and the

earth, and the sea, and the dry land" (Haggai), "that those things that cannot be shaken may remain" (Epistle to the Hebrews). I do not know in what mood of pessimism I might have stood before you to-day had it not been that ere the dew of youth had dried from off me I made friends with the sinless Son of Man, who is the well-head of the stream that vitalises all advancing civilisation, and who claims to be The First and The Last, and the Living One who was dead, and is alive for evermore, and has the keys of Death and the Unseen. My experience compels me to own that claim. For to me, as to the Reformers who founded this University, and to a countless throng throughout the centuries of all sorts and conditions of men, He has established a vivid and vivifying correspondence with our super-sensuous environment. He has made us "see" that at the heart of all things there is a Father's heart. He has made us "know" that in the complex play of circumstances the reins of progress are in the hands of a Circumstant who makes all things work together for our good. Held by His pierced hand, I have witnessed on my way through the shifting panorama of a shaking time some tremendous transformations. These have taken place under the influence of two factors. There is, *first*, the extended range of observation that has been opened to sense, largely by electricity and the spectrum; and *second*, the guidance that has been given to imagination by the doctrine of Evolution. The outer man has acquired instruments of precision, the inner has been given a clue to guide him through the maze of thought. As Science has been seeing more clearly into the constitution and course of things, Philosophy has been able to give a clearer interpretation of their meaning. Let me try in seven paragraphs to reproduce and throw on the screen of imagination some of the transformations I have been made to see.

A PHYSICAL TRINITY.

1. There has emerged from the laboratory of the latter half of the century a luminous trinity. When I first heard of Ether, it was as of a finer air around our atmosphere that came to us rippling with the rays of light. You have been taught to think of it as the something which no man has seen, and

that yet fills all space, is present in all things, and holds all in its embrace. This basal postulate of modern science is arrived at from the study of Matter, which in its ultimate analysis is ether becoming embodied, and which in its turn displays itself under the influence of the third member of the trinity that we recognise as Power or Energy. When I attended a class of Physics, we were told of various forces which were each distinct and capable of producing their specific changes. In 1862, the wife of an Anglican dignitary rewarded me for such attention as an assistant may give to one of his master's patients, by giving me a copy of the fourth edition of Grove's "Correlation of Physical Forces," a work that marked the transition from the age of diverse powers at work in nature to the conception of one continuous energy with varied operations. If I lengthen this paragraph by adding that our textbook in geology was written by Sir Charles Lyell, who was then leading the Uniformitarians in their battle with the Catastrophists, and leading them to victory, and that at his side rose up Charles Darwin to replace the intrusionism that had reigned in biology with the more thinkable theory of evolutionism, you will see that it is now as impossible for my mind to be satisfied with the thoughts that suited it in my student days, as it would be for my body to put on the jacket I laid aside when as a boy of fifteen I left school for the University.

THE EARTH GROWN OLD—

2. I have seen the earth grow old. Brought up to believe that it had been in existence only some six thousand years, I know to-day that its age is to be reckoned by thousands of thousands, not of years, but of ages.

—AND STILL IN THE MAKING.

3. We believed that in six days out of chaos it had been given its final form and furnishing. If one poet could sing how all "at once the lion and the worm sprung from the teeming earth," another could picture the lion as he emerged rampant from the clods, "pawing to get free his hinder parts." The mountains passed for everlasting, and time wrote no

wrinkle on the ocean's brow. You have arrived on a planet that is still in the making. From some nebula in the far dateless past your world has slowly taken shape and course among other worlds, whose interstellar spaces the photograph shows you to be full of stars unseen by earlier generations. Not the sea only but the solid land, all that is of them, and all that is in them and on them and in the air around, are subjects of incessant change, till we can project our thoughts to a far off future, when all may be resolved again into the mist from which could be evolved another cosmos.

THE ELEMENTS DISSOLVED.

4. The very constitution of the world has changed. In the Chemistry class-room, in 1852, a printed board showed a list of the several elements of which the material world was made, with their atomic weights. We were told that some of these might yet be decomposed and their number added to. But we were also taught that whatever new elements might yet be found, they were each distinct and intransmutable, compacted of a crowd of particles of the last imaginable degree of minuteness—the atoms. The atom, the indivisible unit of matter brought from the very verge of thought to guide the chemist in his research, has itself been atomised, and presents itself to us now as an object built up of many parts. It is no longer a solid block, but an edifice—a laboratory if you will—into which we can go and see business transacted among the multitude of ions or electrons where physics is feign to borrow the language of biology for its expression. We seem to ourselves to have come into the borderland where the homogeneous first becomes heterogeneous, where the invisible takes on visibility, where ether clothes itself with matter, and where we are definitely told we may see the mutation of one element into another, so that when we have entered an atom that bore over the entrance the sign of a radium workshop we come out by a door called helium.

PARTHENOGENESIS OF LIFE.

5. The unit of biology has also been transformed. The cell was becoming visible under the microscope about the time

when I was born. My master, John Goodsir, was one of the first to see and state its place in the economy of nature. Another of my teachers, Rudolf Virchow, gave currency to the doctrine of *omnis cellula e cellulâ*, especially through his epoch-marking work on Cellular Pathology, which was "Englished" by a man who listened to his lectures at the same time with me. For a generation the cell, with its wall and its contents, its nucleus and nucleolus, its dividings and its differentiations, was the ultimate object of research and reasoning for the biologist. It has not abjured its dignity in the category of thought and observation. But we see the simplest expression of life to-day in a speck of life-stuff that we call bioplasm. In thought we pass with ease through the cell wall to look more closely at its kernel. Right into the substance of the nucleus we press and see threads of a life-web resolving into chromosomes, which hold and can transmit the peculiarities of various forms of life. And the plasm dot of a chromosome again resolves itself into ids with their determinants and biophors, till we may well imagine we have come into a vast cathedral, from whose roof there echoes to us the sound of the voice of one of our brethren of the centuries B.C., of whom Addison tells us that he called his treatise on Anatomy "a hymn to the Supreme Being."

THE CONTINUITY OF LIFE.

6. It is here, perhaps, that we find the most thrilling of all the transformations that have taken place in the thoughts of those who have the gift, the right, and the responsibility to think. Man, "servant and interpreter of nature," soaring among the stars to analyse their elements, or searching out the secrets of the atom and the cell, has found himself an item in a social cosmos, subject throughout all its bounds to the reign of law. A composite being, formed of the common elements and capable of communing with the Spirit who has not, at any time, anywhere, among any people, left Himself without a witness, he finds that there is nothing alien to him in all the universe. Standing on the summits of creation, summing up in himself the last results of the long processes of evolution, he looks to find his ultimate home in One who is from everlasting

to everlasting God. He is the more encouraged to such lofty ideal when he traces his genealogy back, away back to where the foundations of the earth were laid. His mind reflects the mind that fashioned its materials, and somewhere among the ions and the biophors, where he can scarcely tell whether the ion is not already instinct with more than electricity, or the biophor holds less than life, he sees the narrow fissure between the unseen and the seen, between the not-living and the living, bridged by a kind of virgin-birth, with repetitions of which in some form natural science will make him familiar as he traces the various stages in his own evolution. For parthenogenesis is found not only among the early forms of life, where the distinction between plant and animal is hard to decipher, and where the non-sentient is passing into the sentient, but again and again at transition pages in the evolution story, where we see changes taking place in the types with which the record has been written.

THE INDIVIDUAL LIFE.

7. Each individual from out the race is set before himself distinct from all, yet consociate with all. Our first sight of our individual selves, as we emerge upon the field of being, is as a microscopic speck of protoplasm. It may not differ in appearance or in composition from such another speck as would grow into a creature of another kind. It may resemble the creatures we call protozoa, because in the school of life they have never gone beyond the alphabet. But it possesses the attribute common to everything that lives, of taking into itself what it finds needful from its surrounding elements. Let it respond to the impulses, the claims, of its environment, and use the matter it assimilates to grow or multiply, and it will live on; it will develop and reproduce according to its kind. Let it at any stage of its history begin to assimilate for assimilation's sake, and it is ready to die; or worse, it may degenerate. The microscopic morsel of germ-plasm out of which any of us grew was one of a cityful of more than seventy thousand primordial ova, only four or five of which were selected to pass through evolutionary processes, that recapitulated at various stages the history of their pre-human

genealogy, before they came to claim a place in a nation's register. Each of the four, as his evolution still progresses, finds after he has come to himself, and so long as his self-consciousness abides, that the material part of him is subject to ceaseless change. He is continually putting off his mortal vesture and replacing it with a better fitting garb. The seven pounds weight of matter that was laid in the infant balance passes all away, every atom of it; and if he reach the Psalmist's term of threescore years and ten, he is told that, though he may not have noticed it, he has ten times changed the fabric of the frame his spirit tenants. If he be wise, he will long ere then have pondered how the last fashion of his vestment will be put off—whether in one way or another.

"NOT THAT I WOULD BE UNCLOTHED."

In one way. One of the heroes of my opening manhood, one of the most lovable of men, was so great a naturalist that when he died in his prime, Sydney Dobell wrote me a note suggesting these four lines for his epitaph:—

> "He courted Nature for his mistress; wooed her so
> He won her, till, by love made bold,
> She showed him more than mortal man should know—
> Then slew him, lest her secrets should be told."

A few hours before his passing a friend came to his bedside and asked him, "How do you feel now?" and was answered, "I feel just like a creature carried down a river."

In that way, or in this. When I went to practise in Glasgow the man at the top of the profession, though still in middle life, had resigned the Chair of the Practice of Physic in the Andersonian University, being somewhat delicate in health, and having reached that busy eminence where his fellow-citizens could not let their sick friends die till he had seen them, and where his professional brethren were not quite satisfied that Medicine had done its last and best till they had had a consultation with him. When Dr Andrew Anderson was on his death-bed, one of his fellow-elders, who was also ill, sent to enquire how it fared with him. "Tell him," said Dr Anderson, borrowing from Hopeful in "The Pilgrim's Progress,"

"Tell him I'm in the very middle of the river, but I feel the bottom and it is good."

VALETE.

It may chance that some July day far down the century, when I have long been in the ether, one or other of you will talk with child or grandchild of the years when the century was young. Among its unforgotten scenes there will rise before your mind the memory of the day when at last you burst the chrysalis shell of pupilage to lift free wings into the azure. You will recall the unusual concurrence of the simultaneous leave-taking of the University by the graduates and their promoter. "We came away," you will say to the child, "a goodly company, all together, through the gateway that leads to the rosy dawn. He passed out, all alone, through the door that looks to the sunset and evening star. He was an old man like me," I fore-hear you say. "Not in himself a great man, he had been a friend of great men, and came out of a great time in the nineteenth century 'when there was mid-sea and the mighty things,' and it looked to the men of his generation as if old things had all passed away and a new world begun. And he told us that the great lesson he had learned on his way through life was the same that the disciple who leaned on Jesus' breast at supper taught to the fathers, the young men, and the little children of his time, when He said, 'The world passeth away, and the lust thereof; but he that doeth the will of God abideth for ever.'"

Farewell.

AFTER THE ADDRESS, according to the *Scotsman* report, Mr H. J. Norman, President of the Students' Union, presented Professor Simpson with an illuminated address from the students and the new graduates. In doing so, he said they felt that in losing Professor Simpson they were indeed losing a great and good man. He might speak at length of what Professor Simpson had done in the city and the University, and of the philanthropy which had characterised his life, but his work in that connection was sufficiently well known. The students had experienced the kindliness and courtesy of Professor Simpson at all times, and had enjoyed the hospitality of his own house, and that courtesy had been extended regardless of creed or caste, of race or colour. It must be pleasant indeed for one to feel at the end of such a long period of service that he had fulfilled his duty to the satisfaction of himself and of his *Alma Mater*, but it was a much greater thing to have fulfilled that duty to the satisfaction of the students. He concluded by reading the following address, which the students asked Professor Simpson to accept as a small token of their love, admiration, and respect:—

"*To Professor Alexander Russell Simpson, M.D.,*
"*Dean of the Faculty of Medicine.*

"In the name of the students and recent graduates of Edinburgh University, we desire on this occasion to express our high appreciation of the good work which you have done for our *Alma Mater* and for the students thereof during a period of thirty-five years in which you have held the Chair of Midwifery. We have heard with regret of your resignation of the Chair. You have ably occupied it, and you have added so much to the renown of our University in regard to the teaching and advancement of Midwifery and Gynecology that, in conjunction with your distinguished predecessor, you have caused the name of Simpson in this department of science to rank worthily alongside that of Monro in Anatomy and of Gregory in the realms of Medicine. This you have achieved, not only by your learned contributions to the literature of

your subject, containing many and stimulating suggestions, but also by your practice in the perfecting of technique and in the invention and improvement of the instruments of your craft. Your skill as an operator has redounded to the credit of our school, not only at home, but everywhere abroad, and has not merely maintained, but immensely added to, the fame of our *Alma Mater*. We have to thank you likewise—and this not merely for ourselves, but for the generations of students who have gone before us—for the unflagging interest which you have shown in all matters pertaining to student life. Your desire to foster personal communication between teacher and taught, and your efforts to accomplish this, have earned for you the gratitude and kindly wishes of many thousands of students scattered over the whole world. We hope that, though you now sever your official connection with the University, you will nevertheless continue to take that interest in its and in our affairs which you have so generously exhibited in the past. We pray that you may long continue to enjoy that peace in retirement which no one has better deserved than yourself.

"In name and by authority of the Medical Faculty Committee of the Students' Representative Council.

(Sgd.) "HUBERT J. NORMAN, *Convener*.
"A. H. SCOTT, *Secretary*."

The Vice-Chancellor said: Graduates, I cannot but feel that this most interesting ceremonial would be incomplete if a word or two were not said from the Chair on behalf of my colleagues in the professoriate. Allow me to say these few words, not only because I have occupied the Chair during the ceremony, but because I am one of the oldest friends of Professor Simpson. I knew him as a boy; I knew him when he came to this University in his teens; I knew him when he was an apprentice to that consummate anatomist John Goodsir, whose principal assistant I was at the time; I knew him when he took his degree of Doctor of Medicine; and I have been his colleague for thirty-five years. Now, I look upon this great assembly as something more than an assembly of persons interested in you graduates; I look upon it also as a testimony of respect and esteem for the colleague that we are about

to lose. I regard it as a magnificent send-off. When a friend is about to sail for a distant clime, his friends often flock to the steamboat quay to say farewell. Our friend, Professor Simpson, is going to seek during his latter years rest and retirement at the end of a long professorial life, and I feel that many of you have come to give him a good send-off in that rest and retirement of which he now intends to avail himself. Mr Norman has referred to the eloquent address which we have just heard from Professor Simpson, but I feel that there is something more than the mere eloquence of words in that address. There was a deep feeling in it—a feeling of a kind which it is not perhaps very easy to refer to; but it comes from the heart—it comes from one whose life has been actuated by a strong sense of duty, who has lived amongst his colleagues and his students as one who always felt that he had a task in life to discharge, and he discharged it with consummate ability. And so we, his colleagues in the Senatus, feel that we cannot allow him to separate from us on this occasion without wishing him during the rest of his life—and from what I know of him, I am sure it will not be an idle or a wasted life—health and strength to carry on that kind of work which he so well knows how to execute.

Professor Simpson, who was cordially received on rising to reply, said he had to thank them with feelings that were too deep for words. He had said that it was not without a pang that he left the place he had filled so long. He should have said that it cost him a great many pangs to leave all those friends of his youth, of his manhood, and now of his old age. But, as he had said, the deepest sorrow was for a man to be leaving his subject just at the time it was becoming more intensely interesting. There was, however, the compensation of knowing that younger men were coming—Mr Norman and his compeers were coming—and he left to them to pursue the subject, in which they would win laurels brighter perhaps than those that had adorned the brows of their predecessors.

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THE SOUTH AFRICAN MEETING OF THE BRITISH ASSOCIATION.

THE TOUR OF THE ASSOCIATION.

By Professor J. Y. SIMPSON, D.Sc.

(*With Illustrations.*)

To tell the story of the British Association in South Africa is to furnish an itinerary of nearly twenty thousand miles of wandering, and to furnish *résumés* of numerous papers of first-rate importance, dealing either with local problems of general interest or with the most recent advances in modern science. It is also to recount the lighter side of the most gigantic picnic ever organised, and unstintedly to shower praise on many officials at home and in the colonies who worked unsparingly to make the visit the unqualified success it ultimately proved to be. When one considers what was involved in the transport and provisioning of four hundred men and women, particularly in territory where even the settlers are still feeling their way, it would have been small wonder had there been failures or breaks-down to record. That nothing of any moment occurred is ample proof of the care with which every detail of the Association's progress through the country had been arranged; and if one or two aldermanic individuals have searched for and found grounds of complaint, it is principally due to the fact that they failed to recognise that place and position at British Association enterprises and functions bear no direct relation to a man's waist measurement.

The bulk of the official party landed off the *Saxon* on August 15th, and the same evening the business of the Association commenced. Three days were occupied in this way, after which a dispersal took place, with Durban as the rendezvous. Some went directly there by steamer, visiting Port Elizabeth and East London *en route*; others remained to take advantage of the excursions that had been planned in the neighbourhood of Cape Town; geologists moved off immediately and instinctively in the direction of the Karoo; a few selected the overland route, with its bird's-eye views of Cape Colony, the Orange Colony, a portion of the Transvaal, and Natal.

With regard to Cape Colony, it must be admitted that the region is at present passing through a severe economic crisis. Trade is generally seeking those ports that involve a shorter railroad journey to the dominating Transvaal. The bulk of the country is pastoral, yet, owing to the periodic, often years-long, droughts, farmers have sometimes been compelled to slaughter their lambs because of the insufficiency of food for them and for the sheep. For the same reason agricultural pursuits are hindered beyond a certain point, nor is there any prospect of irrigation on a scale comparable to that in India, simply because there are not the rivers. Locusts descend in clouds, and on the wheat-patch off which the farmer hoped to garner sixty bags, not a blade is left. Owing to rust, only two months' supply is being grown at present in place of a sufficiency for seven months; hence follows the need for importation, but the diffi-

culties of labour and of water are so great that the local farmers cannot compete with the imported grain. Worcester was the centre of viticulture on a large scale, but the Phylloxera has brought the industry into a very parlous condition, and it must be long before the local varieties have everywhere been grafted on to introduced American vines. As for stock diseases, their name is legion.

Much of this is patent even in the rapid view that was necessarily the lot of the British Association member; but, on the other hand, evidence was not far to seek of the success that has attended the Government endeavours to develop portions of the Karoo by boring for water, as at Matjesfontein, 195 miles from Cape Town, on the Western

FIG. 1.—On the Karoo.

Railway system, and Laingsburg, 18 miles further along the same route. Again, systematic campaigns have been planned against the locusts, and by means of spraying with arsenate of soda and other devices, the Agricultural Department, with the willing assistance of the farmers, has done much to reduce this evil, while every day fresh information is being gained as to the best manner of dealing with the various internal and external parasites that are the active causes of the horse and cattle plagues.

The Karoo once gained by way of the beautiful Hex River Pass, gives the impression of a vast waste; mile upon mile of light brown sandy soil, through which at points the bare rock shows, like a skeleton breaking through the tight-drawn mantling skin. And that same soil is little concealed by the very discontinuous growth of heath and shrubs

—mesembryanthemum and euphorbias for the most part, together with *Acacia horrida*. There is nothing to rest the eye; everywhere hard, straight lines even on the summits of the occasional kopjes, and a general feeling as if one were crossing some enormous Highland stony fallow field.

In and around Durban the Association had the opportunity of seeing something of a really British colony, as of the two industries that have done so much to make it commercially successful. Already 4000 acres are under cultivation to tea planters, while the sugar industry antedates the history of the country as a British colony, and is now capable of putting £600,000 worth of produce on the market annually, as also of giving employment to 8000 people. Of these, however, the great majority are

FIG. 2.—Marriage of Zulu Chief, Henley, Natal.

imported Indian coolies, who, together with those employed in other lines of service, now outnumber the 97,000 Europeans, and, being free, constitute something of an Indian peril. For by their industry, sobriety, and economical instincts they practically have all the smaller trade in their hands, while many of their number are rich both in money and in land as the result of business efforts on a grander scale.

On more than one occasion native war-dances were arranged for the entertainment of the visitors, and there are those of the over-sea party whose most vivid impressions were gained on the 25th of August, when at Henley, in the vicinity of Pietermaritzburg, the chiefs and representatives of these large tribes, who number collectively over 18,000 souls, gathered to do honour to the Governor of the colony, and take part in the wedding festivities of one of the chiefs. One of the illustrations

accompanying these notes represents a preliminary stage in the order of procedure, when the agent of the bridegroom, usually a brother or uncle, who has arranged the marriage and the amount of the marriage consideration or obolo, steps forward and, after a speech to the members of the tribe, shows his pleasure by performing a number of antics and evolutions: the bridegroom is seated on the ground in the centre of the circle. Certain customs usually observed at these tribal weddings are interesting, and might even appear to some Western minds worthy of imitation. Thus the visitors were credibly informed "that the mother of the bride, on no account whatever, may be present at the wedding; that if the bride is the eldest daughter of her father, he does not attend at

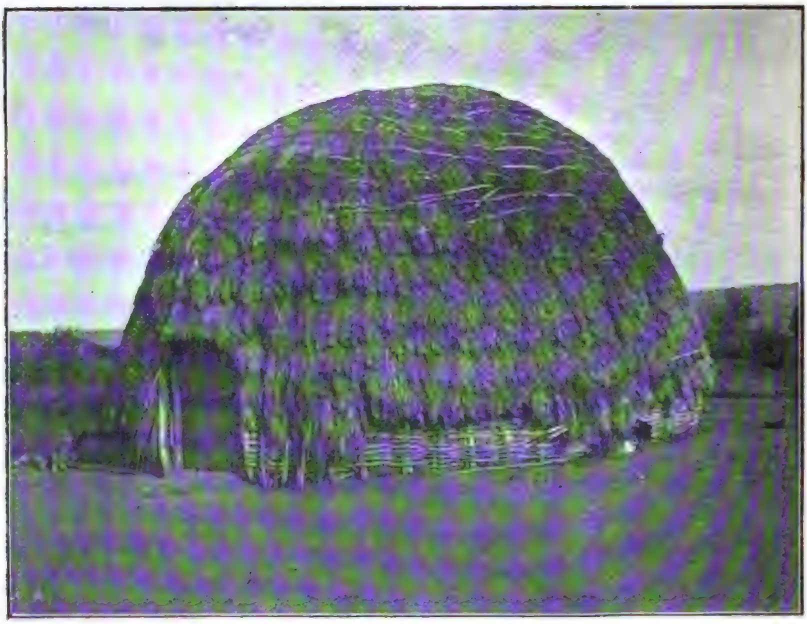


FIG. 3.—Native hut, Bellairs, Natal.

the wedding; that the bride may not look at the husband's father; that in many tribes the mother-in-law may not look at her son-in-law, nor may the son-in-law look at her." But, added our informant, "these usages vary, and are becoming more or less obsolete with the advance of civilisation."

Several opportunities presented themselves of seeing the native in his home, both in small country kraals and in the larger stadts that lie on the outskirts of most South African towns. It became evident that the native question is *the* South African problem. Each of the four colonies has its specific difficulties, but in every one there is this pressing matter in one form or another. The difficulty is increased by the fact that in each of the colonies the attitude of the white man towards the native differs, varying from the extreme Cape Colony position, where he

is given a vote, to, say, the Transvaal point of view, where he is not even allowed to walk on the pavement. A principal obstacle to the South African Federation is the present impossibility of agreeing on a uniform attitude to be observed towards the native. One thing, however, stands out clearly, and that is the necessity of teaching the Kaffir the dignity of labour, together with some regard not merely for his fellows, but for the land in which he lives.

The hours spent on the Natal battlefields in the course of the Association's progress towards Johannesburg are a source of very potent memories. The bare grave-strewn field of Colenso; the actual position of each of Long's guns marked by a stone only recently removed, as if men wished that the incident should be forgotten; the flat-topped Hlangwane deceiving everybody as it deceived Buller into thinking that it lay on the other side of the Tugela; the Boer trenches with the barbed wire still in place, nay, with the very pebble-filled tins still attached that would have rattled out the advent of the enemy; bullets and fragments of shrapnel lying around—it was all terribly real even after five years. As also Ladysmith, back of the hills, defended by a miracle, and for ever a source of inspiration to every one who regards the flag of his country with affection.

"Tell England, ye who pass this monument,
We, who died saving her, rest here content."

Such are the lines on a soldier's tombstone in the Wagon Hill cemetery.

In Johannesburg three days were spent in scientific session, but plenty of times and occasions presented themselves for making cursory examinations of the mining industry, and for inquiring into such a burning question as Chinese labour. There is little doubt that one great benefit of the Association's visit to South Africa lies simply in the fact that a number of intelligent British men and women were enabled to see vexed matters on the spot and obtain information at first-hand, which was bound profoundly to modify any preconceived ideas on such topics as the native question, the education question, and Chinese labour. With regard to the last point, many of us came unhesitatingly to the conclusion that, although an evil, it is yet at this stage a necessary evil. Further, it is the Transvaaler's question, and with him must rest the decision; for it is he who suffers, and it is he who also, whatever may be said to the contrary, will benefit by the presence of the Chinese labourer.

After the Johannesburg sojourn (in connection with which was a delightful visit to Pretoria), the Associationist found little rest for the sole of his foot. The bulk of the party went round by rail by way of Bloemfontein and Kimberley to Bulawayo: a few took the opportunity of seeing the country and the Boer at home by trekking direct to Mafeking. This meant a journey of five days by mule, wagon, or coach, over territory whose characteristic features changed daily, and finally blossomed out as "The Garden of the Transvaal" in the Marico district. Although there is still considerable dissatisfaction and bitterness amongst many

of the Dutch farmers—and that not altogether without reason—yet it is probable that the majority are well disposed if only they are sympathetically and fairly treated. Nothing seemed to be more clear than the wonderful success of Lord Selborne in a most difficult position, and it is sincerely to be hoped that he will be given every opportunity of completing the work which he has so auspiciously begun.

Bulawayo, founded on sentiment, is at the present moment finding in it poor sustenance. It was a Rhodesian idea to plant a town on the site of Lobengula's kraal, and so long as it remained the terminus of the railway, and the mining industry was in its initial stage of unjustifiable inflation, the place "boomed," but a day came when the

FIG. 4.—"Drift" on the road, Marico district, Transvaal.

railway passed beyond the town, since when it has been gradually retrogressing, although it is probable that the backward movement has now reached its limit. And in any case Bulawayo must ever be a place of pilgrimage, for in its vicinity lies the wonderful Matopo country in the middle of which Cecil John Rhodes is buried. The outlook known as the "World's View" is unique; as if one gazed upon the unswept workshop of creation, the place where the earth was fashioned in the days of old—ridge upon ridge of fantastically-shaped granitic masses lying about in strangest confusion.

Our Ultima Thule was the Victoria Falls, where, however, sufficient time was hardly allowed to gain a complete impression of this glory of Nature. Under the circumstances, however, this was perhaps unavoidable, and only served to deepen the intensity of the moments that

were available. The tropical vegetation of the river banks and islands, the clear and placid water above the Falls, the sudden frenzied leap into the mile-broad chasm, the tumult of the waters hidden in the impenetrable barrier of spray and spindrift rising hundreds of feet into the air, the beauties of the gorge, and a hundred other points of appeal—no man may easily reproduce them with pencil or with brush. We retraced our steps to Bulawayo. Thereafter some returned directly by Cape Town, while others, journeying east by Salisbury and Umtali, embarked at Beira. Thence, touching at Mozambique and Mombasa, where time only permitted a short but romantic run up the British East African Railway as far as Mazeras, the eastern detachment of the Association suffered compulsory but not altogether unrewarded delay in the Suez Canal owing to the *Chatham* misadventure, eventually, however, with the loss of three days, reaching that country from which, when any inhabitant sets out, he somehow thinks of the day of his return.

With the Author's Compliments.

To A. E. S from

14 04

15

"A GLORIOUS CONQUEST FOR HUMANITY."

"The fierce extremity of suffering has been steeped in the waters of forgetfulness, and the deepest furrow in the knitted brow of agony has been smoothed for ever."—O. W. HOLMES.

Some Pioneers of Anæsthetics.

[Friar Laurence, R. & J. IV. 3.]

NITROUS OXIDE.

Joseph Priestley - - - - - - 1776
Humphry Davy - - - - - - 1800
Horace Wells [Collyer, Colton, Riggs, Evans, Bert.] - - 1844

SULPHURIC ETHER.

M. Faraday (?) - - - - - - - I
W. T. G. Morton [On himself and on Eben. H. Frost] - - 1846

"Before Whom, in all time, Surgery was Agony
Since Whom Science has control of Pain."

J. C. Warren [On Gilbert Abbott, 20, Painter, Single] - - 1846

[Wilhite, Long, Jackson, Hayward, Bigelow, Boot, Robinson, Liston, Buchanan, Longet, John Snow, Simpson, Bernard, Clover.]

CHLOROFORM.

James Young Simpson - - - - - 1847

"I'll imitate the pities of old Surgeons
To this lost limb—who 'ere they show their art
Cast me asleep, then cut the diseas'd part."

T. MIDDLETON, "Women Beware Women," iv. 1, 1657.

[Guthrie, Soubieran, Liebig (1831), Dumas (1834), Waldie, Flourens, G. Keith, M. Duncan, Snow, Nunneley, James Arnott.]

IN HONOUR OF

The Victoria Dental Hospital,

SEPTEMBER 30th, 1902,

And in Memory of

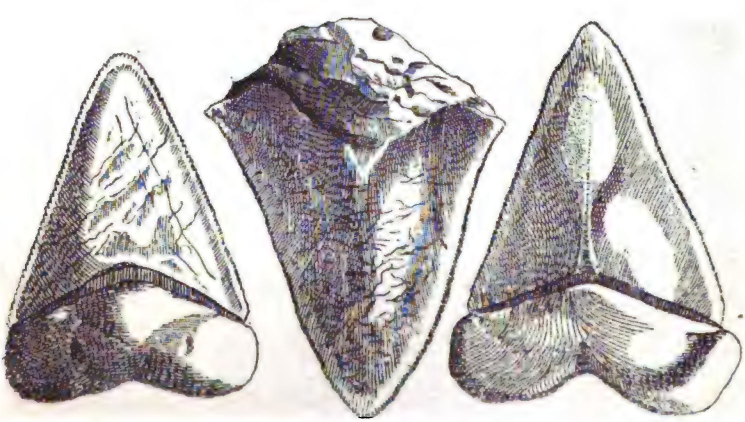
September 30th and October 16th, 1846, Boston, U.S.A.

TAB. IV. fol. 90

LAMIAE PISCIS. CAPVT.

TAB. VI. fol. 134

GLOSSOPETRÆ. MAIORES.



NICOLAI STENONIS.—" *De Solido intra Solidum,*" *etc. Ad Serenissimum Ferdinandum II. Magnum Etruriæ Ducem. Florentiæ,* 1669.

" *Sténon était un homme de génie. Deluc l'appelle le premier vrai géologue. Il a commencé l'anatomie du cerveau et il a commencé la géologie.*"—FLOURENS.

1638—1686.

" *Observationes anatomicæ de glandulis oris,*" *Lugd. Bat. 1662.* " *De cerebre anatome.*" *Elementorum myologiæ specimen ; cui accedunt canis carchariæ dissectum caput, et dissectus picis ex canum genere, Amstel., 1669.*

Copied from a print by Vilhelm Maar, taken from a portrait in the Galleria Uffizi in Florence, Circa 1669.

Ferdinandum II. Magnum Etruriæ Ducem. Florentiæ, 1669.

JOHN HUNTER.

1728–1793.

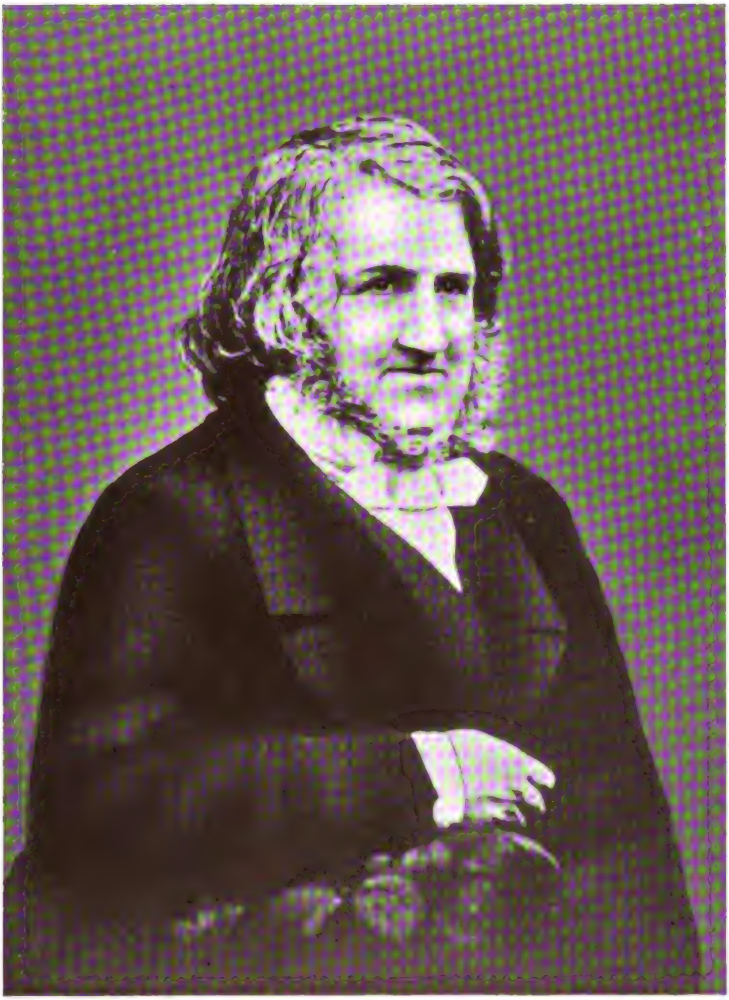
"*In that masterpiece of portraiture, which teaches like a chapter of biography, Hunter is not shown as the busy anatomist or experimenter pursuing objective facts; the chief records of his work are in the background; he is at rest and looking out, but as one who is looking far beyond and away from things visible into a world of truth and law which can be only intellectually discerned. The clear vision of that world was his reward. It may be the reward of all who will live the scientific life with the same devotion and simplicity.*"—Sir James Paget, 1877.

. Magnum Etruriæ Ducem. Florentiæ, 1669.

GENESIS ii. 21.

"*Dolor dolentibus inutile est.*"—[GALEN.]

Sir JAMES YOUNG SIMPSON.



1811—1870.

"*He stingless left the old Semitic curse.*"

"ANÆSTHESIA."

Aet. 17.

Ferdinandum II. Magnum Etruriæ Ducem. Florentiæ, 1669.

ALPHA.

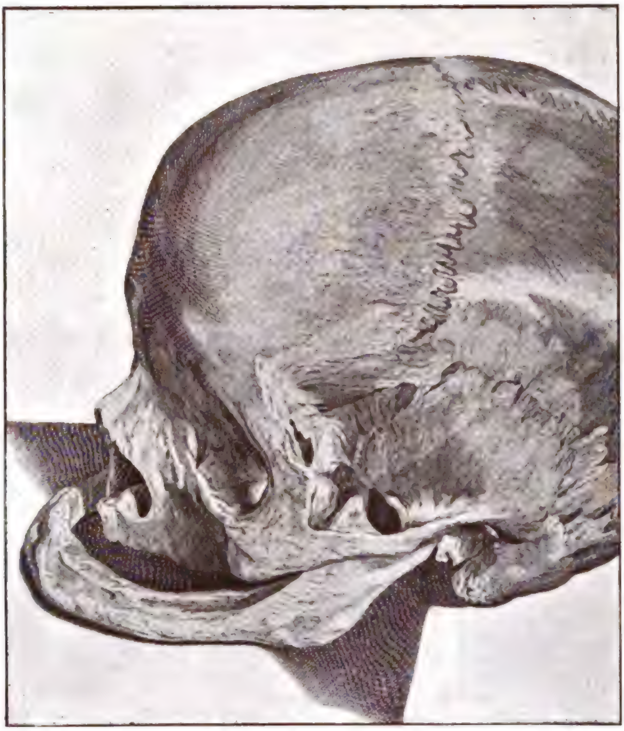
OMEGA.

Reproduced from a print in the possession of Walter Whitehead.

V. C. **V. P.**

15/IX./02.

W. W. L. M. W. S.



(From " The Natural History of the Human Teeth," by John Hunter, London, 1778.)

"Sans teeth, sans eyes, sans taste, sans everything."

Ferdinandum II. Magnum Etruriæ Ducem. Florentiæ, 1669.

TH. BARTHOLINUS.

■ ■ ■

Quod felix faustumque sit et mortalibus salutare!

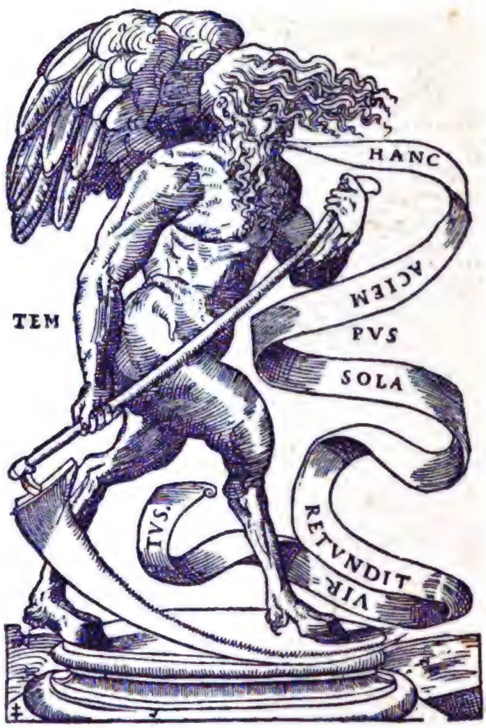
Favente Summo Numine, Jubente Clem. Rege, Annuente Perillustri Cancellario, Consentiente denique Magnifico Dn. Rectore et Facultate medica Th. Bartholinus D. et P. P. cadaveris virilis dissectionem crastina luce, die . . Ann. . . hor. 1 pomerid. auspicaturus sequentibusque diebus, si Deus, si valetudo convenerit, continuaturus, omnium ordinum Auditores, spectatores, auditum ut veniant et spectatum quibus volupe, qui se mortales sciunt et cognoscere amant, qui illustre Anatomes studium suspiciunt, qui se suamque corporis Majestatem amant, in Theatrum Anatomicum summo, quo potest, officio et pari humanitate invitat, convocati.

May this be fortunate, prosperous, and health-giving to mortals!

With the favour of the Supreme Being, at the bidding of our Gracious King, with the approval of the Illustrious Chancellor, and finally with the consent of the High Rector and the Medical Faculty, Th. Bartholinus, Med. Doct. et Prof., who will commence his dissection of a male corpse to-morrow, the   of   at 1 p.m., and will continue it on the following days if God and his health permit, invites hearers and spectators of any rank to come to hear and see, who care to do so, who know that they are mortal and love to learn, and who reverence the illustrious study of Anatomy, and love themselves and their own physical dignity—called together into the theatre of Anatomy, with the highest possible respect and equal courtesy.

Translated by request by Professor A. S. WILKINS.

[*From Carolus Stephanus, 1545.*]



With the Compliments of

William Stirling, M.D., D.Sc.,

AND

With personal thanks to W. Brown and J. Spencer.

16

"*The*
Relation *of* University Education
to Commerce"

With personal thanks to W. Brown and J. Spencer.

COMMERCIAL CLUB
OF CHICAGO

"*The* Relation *of* University Education *to* Commerce"

ADDRESS BY

WOODROW WILSON, LL. D.
PRESIDENT OF PRINCETON UNIVERSITY

NOVEMBER THE TWENTY-NINTH
MCMII

With personal thanks to W. Brown and J. Spencer.

Commercial Club of Chicago

THE ONE HUNDRED AND SEVENTY-FIRST REGULAR MEETING
WAS HELD ON NOVEMBER THE TWENTY-NINTH,
NINETEEN HUNDRED AND TWO

The President, Mr. David B. Jones, *presiding.*

SUBJECT:

"*The Relation of University Education to Commerce*"

Address by DR. WOODROW WILSON,
President Princeton University.

THE PRESIDENT: It is no mere accident that the Commercial Club of Chicago has as its guest to-night the newly elected President of one of our oldest institutions of learning. I shall not surprise him in saying that "Commerce is king." It has brought under its influence the kings and monarchs of the world. By that slow, coercive power which all great forces exert, it has exercised an influence in planning war or insisting upon peace which no monarch or no king possesses. It also is a civilizing power which I think can challenge even the educational institutions of the world. We have, I think, the highest academic authority in this country for the statement that work—and that involves commerce;—that work itself is the most civilizing agency in the world. It is a statement recently made by President Eliot of

through a two-inch board. She got up in great excitement, and said, "Let me out of this place. This is no place for me to be with these thin things on." (Laughter.) I should feel very much in her position if I should attempt to pose before you as an authority on commerce. But whether I am an authority on the relation of the university to commerce or not, I hope soon to qualify myself as an authority and I am trying myself off on you. This is my maiden trip in this capacity, and I want to read in your faces, if I may, whether I am right or wrong in the position which has seemed to my thought a valid position.

In the first place, it does not seem to me that a university is a place to give a man a business education. A druggist in a very small way at Princeton—all merchandise is in a small way in that small place—took me aside the other day, and told me now was the opportunity to establish at Princeton a business education. And when I questioned him as to what he meant by a business education, I understood him to mean the keeping of accounts and the handling of commercial paper, and the technical duties of commercial transactions. I told him that it did not seem to me that we were quite ready yet to transform Princeton into a business college; and I believe that you will all agree with me that the real point of commerce is not the method of commerce, but the catholicity of its outlook. Commerce is great or small according to its horizon, not according to its detailed method. No man ever made a fortune out of method. No man ever extended commerce by method. He extends commerce, I take it, by vision; by an imaginative conception of places and of conditions which he never saw; by having what I may de-

scribe as a traveled mind, which has gone up and down the world and ascertained the conditions of men; and there never was a time, assuredly, when it was more necessary for the commercial mind to be a traveled mind than at the present day, when commerce is almost of necessity international in its scope; where no neighborhood marks its boundaries; where the men who are engaged in commerce can hardly set limits to at any rate the imaginative conception of what commerce will one dav become. And when we address ourselves to questions like the Chinese question, we are really asking ourselves what is the means of entrance of western ideas into eastern countries. We know instinctively that the western ideas go in with the western goods, but that first of all the western goods must fit the eastern ideas; that you are not going to get your entrance into eastern markets by forcing at the outset western tastes upon the eastern buyers; but that you will get your eastern markets by understanding eastern tastes, so that your business as traders with the east is to understand not your own minds so much as the eastern mind, as the eastern taste, as the very religion of the east, so that you may know those prejudices which are hardened against change, those tastes which can be invaded only by a sort of sacrilege; those things which your thoughts must know and must match if you would achieve ascendancy in the markets of the east. And so it seems to me that it is true that it is the traveled mind that knows the markets.

One of the most characteristic things of modern trade, I suppose, is the commercial traveler. I don't know of any more impossible person than some commercial travelers that I have met. Because some commercial travelers—men

without elasticity of mind who believe that they can through their single, individual eye, read all the world as they travel—are more safely insured against the introduction of knowledge than any class of men I know. of. (Laughter). A Yale professor of my acquaintance said that the result of his twenty years' experience in teaching was that the human mind has infinite resources for resisting the introduction of knowledge (laughter), and I believe that those resources are increased by knocking about the world with a dull mind. The mind gets case-hardened. It gets smoothed over on the surface. It gets all the ordinary touches of life exhausted upon it, and a man thinks that his mind is complete because its surface is fair, although its interior is empty, and he takes the hollow sounds that come from it as being the real music of intelligence.

There are some commercial travelers of that sort. But there are commercial travelers of another sort, who see through things, as well as see the surface of things, and this casting up and down the world, this familiarity with steamers that seek the other side of the globe, this life upon the fleeting tides of what men are doing, is the characteristic feature of modern commercial undertaking. Everybody knows, of course, that modern industrial undertaking strains at the leash all the time to get into channels of commerce, that commerce is the outlet of industrial undertakings and that industrial undertakings are without significance unless they have the outlets of commerce; that the commercial man, the trader, is the middle man between the product and its freedom to command the markets of the world, and that without the intelligence of the

ought perhaps to say one word, before introducing Dr. Wilson, in regard to his personality. A recent critic, in passing judgment upon one of Dr. Wilson's books, said that portions of the book displayed a very narrow spirit, and immediately added, "Dr. Wilson must be a very narrow minded man, otherwise he would not have been chosen as the President of Princeton University." So much in explanation of the personality of the man whom I now have great pleasure in introducing to you. (Applause.)

DR. WOODROW WILSON: Mr. President, and Gentlemen of the Commercial Club: I feel the honor, as I feel the embarrassment, of standing in this place, because I know how many really distinguished men have been your guests, and I know how far I am from having proved my right to stand in this place. But I take it that one of the encouraging signs of our age is that men have that catholic desire to interchange ideas which makes it a welcome thing to them to hear any man who speaks at any rate from conviction and out of a thoughtful mind. And so it is not a presumption on the part of a man placed in the position in which I have recently been placed, to ask leave of you to speak aloud some of the thoughts which have been passing through his own mind in respect of the relation and the responsibility of the university in regard to commerce.

I cannot pose as an authority on commerce. There are gentlemen in this room who know me, and in whose presence it would be embarrassing to pose as an authority on commerce. I should feel in the position of that unsophisticated woman, who, in a side show attached to a circus, saw, or supposed that she saw, a man read a newspaper

Salisbury is that he does not ordinarily think of enough things at a time; he thinks of one thing at a time, and nothing is more fatal in this complex world than to think of but one thing at a time. You have got to think of—by a minimum computation—at least a dozen at once, and Lord Salisbury had his variety cap on that morning, and did not think that a war with the United States was worth while, and so the commercial men of the United States were saved their nervous prostration. But you will observe it is a matter of nerves, and when you say it is a matter of nerves you mean that it is a matter of sympathy, and a matter of sympathy is a matter of imaginative conception. I haven't any sympathy for a man if I cannot form some imaginative conception of the contents of his head. If I don't know what he is thinking about, I don't know what to think about myself in addressing him. One of the most inconvenient things that can happen to a man in my business—that is, of addressing college classes—is to have a dignified and inscrutable stranger enter the class room and sit down (laughter); then you begin to say in your heart, "Who in thunder is that?" (Laughter.) "Is he a merchant?" You can generally tell if he is one of the cloth. But what is he, and what part of this lecture is going to interest him? Because we have a more or less histrionic instinct and we all wish more or less to appeal to the audience which we are addressing, and we instinctively know that that man knows more than any of these boys that are sitting here, and that if we are not particularly in good form that morning we had better brace ourselves up to his standard and not count on the average ignorance of our audience (laugh-

ter). And so it becomes a matter—a very acute matter—of nerves in those circumstances. It is in every case a question of imaginative perception as to how the other man feels when it comes to a question of influence.

Now with all this elaborate setting out of the stage, I introduce the university to play her part. What part is the university to play in preparing men—I won't say in preparing men, but in serving the community in respect of commerce? I have already said that it does not seem to me any part of a university function to give men a business education, to teach them, that is to say, the methods of the business office. There is no touch of the university in that. It always must do something very different from that. I have no doubt that technical schools of various sorts are extremely serviceable, but technical schools of the narrow sort do not seem to me appropriate parts of a university. We are apt to forget, gentlemen, that the university is not intended for everybody. The principle of power is a principle of differentiation. We are in danger just now of supposing that a university must include every kind of education, and we are apt to lose distinctions of thought and efficiency of result by confusing one sort of education with another. There must be various sorts of education, and when I say that the field of the university is set apart, and peculiar, I am not meaning to imply that it is better, that it is more noble, that it is more dignified than other fields of education. I believe—as every man born on this soil, I take it, must believe—that the dignity of toil with the hands, provided the heart goes into the work, provided the conscience gets translated into the product, is enough to

dignify any man and give him a touch of nobility. There is no comparison in point of nobility, there ought to be no attempt to compare, in point of nobility of work, the work of the head and the work of the hands. It is not a question of nobility, it is a question of division of labor, of the separation of functions. It is a question of that differentiation upon which the efficiency of the modern world depends. And upon that basis, not upon the basis of comparison, I say that the business of the university is with the head and not with the hands; that the business of the university is something different from the business of technical education, and if men are so impatient that their sons should get to the immediate tasks by which they make their living that they cannot allow them to spend the four years necessary for a university education, the thing to do is not to send them where they will be pretending that they are in college, but to put them at the desk, and let them learn the business like men. Don't put them in a technical school which is called a university and then say that they are getting a university education. Not because they won't be getting something valuable there, I repeat, but because you must not confuse ideas. And the idea of a university education is different from the idea of a technical education. It seems to me that the thing that the university must do is to make men acquainted with the world intellectually, imaginatively.

The business of the university is to cultivate intellectual imagination. We do not enough emphasize that in our idea of university work. We suppose that a university is a place where the contents of certain books are extracted and inserted into the minds of certain young men, and that the whole process

is a process of taking from one vessel and pouring into another; that it is a process of filling, a process of cramming; that it is a process of informing, informing by authority, of telling a young man that a certain thing is so because it is said to be so by an authoritative text book. That is a very mechanical and a very mistaken idea of a university. Mind you, I don't say that many universities don't do that. I am not saying that most universities don't do a great deal of it. Most of us do a vast deal of it, and it is a mistake to fill a young man's head with statements given him not upon proof but upon authority, for the thing in which you should pride yourself in the teaching function, it seems to me, is the liberation of the human mind from authority; the cultivation—to repeat the phrase I have already used—of intellectual, independent observation. For your purpose is to give these young fellows the freedom of the world of thought and the world of affairs. You will say there is no way of getting at the freedom of affairs except by mixing in affairs. How many men ever mix in many affairs? The most experienced man of affairs takes the knowledge of nine-tenths of the affairs that he knows anything about from the mouths of other men and from the books and official reports that he reads. I remember being very much impressed with that fact not long ago when Mr. Cleveland was writing a couple of papers on the quarrel which many of you will remember that he had with the United States Senate about dismissals from office and appointments to office in the early part of his first term as President. In writing those papers he took occasion to read the congressional debates which occurred in connection with the quarrel, and as I had the privilege of

being with him during a part of the time in which he was preparing these papers, he told me that he was beginning to find out a great many things with regard to the circumstances at that time and of those transactions which he had never known before, "because," he said, "in the daily pressure of executive business, men would come in and tell him that this, that or the other thing had been said in the Senate or in the House; that this, that or the other thing was being rumored on the street; that this, that or the other thing was being done, the shifting of this group and that group of men, and that he did not have time to read anything about it and so had only uncertain information which was brought him by word of mouth." Nobody stood more at the center of that particular transaction than Mr. Cleveland, and yet he knows nine-tenths of what he knows about it from the report of other men, and from the printed records of the debates of Congress. Ask yourselves, sitting in your office, how much you know of what is actually going on in your business. You know it by the reports of the men under you. You know it by knowing that on previous days certain things were done, and the number of things that come under your direct observation as you sit in your office are absolutely insignificant as compared with the number of things which imaginatively come under your observation and are present in your thought as you see, as it were, the map of the business before you. And so it is that it is not idle to claim that the university can spread some portion of the map of life before men, and out of the credible evidence of men who have taken part in affairs, cultivate among men an intellectual and independent observation of the life of mankind. I am now, of course,

talking about undergraduates. I am not talking about advanced students, who set themselves to master a specific task of scholarship. That is another story. That is a matter of the investigation of abstract truth. And I suppose that every intelligent man knows the service that that does. This wonderful instrumentality which is just now in its infancy and will presently run through every process of the globe—I mean electricity—would not have the practical uses that it has now if men had not in secluded places of investigation sought the laws of electrical force without so much as thinking for a moment of the significance of that force from a material and commercial point of view. Commerce has got the by-products of their mind as that mind has moved forward in its eager, unremitted pursuit of abstract truth, essaying to know the laws of nature and the laws of God. Commerce, I say, has stood on one side and picked up the droppings from the tables of these men, and has put it to use in the enrichment of a world—in the material enrichment of a world. That is the service that abstract scholarship does to the material undertakings of men. If men were not bent upon the essential things, the practical thing would not come into revelation at all. But I am not now speaking of that. That is another matter. That is a matter of the general progress of the world. I am now speaking of the administration of the world, and the administration of the world comes by letting men out upon the field of life imaginatively. There are only two ways in which to do that. There are some men who get out upon that field by their own genius, by their own acute power of observation, by the intensity with which they live. You can count those men on the fingers of two

hands in any community. Even self-educated men—the men who have not stood in need of the university—would have been set forward faster, I take it, if their imaginings had been assisted by the right forces of university training. But they have had such intrinsic genius for observation that the university has not been indispensable to them. For the average man the university is indispensable. Not, it may be, to enable him to know these things before he dies, but to enable him to know these things at the outset. The university is, if I may express it in the language of the racetrack, the pace-maker for the mind. It shows the mind the pace that it must strike at the outset if it would keep a winning gait throughout all processes of endeavor. It is a process of informing men of the facts of life. And what we are most apt to forget is that one of the principal facts of life is thought, is the thinking process, is what men have conceived their lives to be,—the purpose that men have conceived to be contained in their lives. For when you think of it, gentlemen, the only imperishable thing that any one of you is creating is the conception of his business. The particular things which you manufacture or deal with are going to pass away from the sight of man; the things which you are creating are invisible. They are conceptions of business. You know that the great business mind is the mind which, when you lay before it certain elements, combines those elements in a situation and sees where to put the money and when to put the money. That is a purely abstract conception.

Nobody ever saw, for example, a government. I mean, with the physical eye. Well, I don't know that that is

literally true, either. At one time when Richmond was evacuated, some gentlemen did see the Confederate government, I believe, on its way beyond the lines. But leaving that singular and exceptional instance out of view, I take it that nobody on any other occasion ever saw a government.

One of the most impressive things that was ever said, though when repeated in cold blood it sounds rhetorical, was what Mr. Garfield said when Mr. Lincoln was assassinated. I am told that at that time Mr. Garfield was at the Fifth Avenue Hotel in New York, and that when the news reached New York an excited crowd gathered in the square in front of the hotel ready for almost any outburst of frenzied excitement; that Mr. Garfield was asked to step up to the portico of the hotel and say something to quiet the crowd, and that this is what he said: "My friends, the President is dead; but the government lives and God omnipotent reigneth." And, as you may readily believe, the crowd was quieted. They knew, whether they analyzed it or not, that that abstract conception about which our loyalty gathers, the facts of law and of government, stood just as firm as if that man still lived; that though that rare spirit, which better than any other spirit then living could handle that government, was gone from among us, the government remained. And it steadied them with an abstraction.

Every man who fights under the flag of the United States fights under an abstraction. He fights for a thing he never saw and never can see. He fights for a poetic idea, and when we speak of the nation, we speak as men who are ignorant, unless we have some imaginative and sympa-

thetic conception of the parts of the country in which we do not live.

You know, gentlemen, as well as I do, that the great incubus upon this country is its provincialism. I mean that this country is still not homogeneous in the makeup of its life, and consequently the makeup of its habits and conceptions, and that one part of the country is apt to look askance at another part; that when there thrills through the air such a question as, "What is the matter with Kansas?" for example, something is wrong; that one part of the country should have to ask what is the matter with another part of the country and not know that it is just as reasonable for Kansas to ask what is the matter with the rest of the country. It is provincial. It is downright provincial not to have an imaginative conception of what the rest of the country is like, and that is the imaginative conception which the university is meant to give to men, not only in regard to the country in which they live, but in regard to the world in which they live. Because a cultivated man is, as I understand it, a man who knows what the work of the world is, and who consequently understands what the relation of his work is to the rest of the work of the world; who finds his zest not so much in the narrow road of daily routine in his office as in his knowledge of the relation which all of that bears to the whole burden of work which the world is carrying.

One of the most encouraging features of our day is that men like to get together and remind each other of the horizons that surround them; that men find business intolerable unless they can meet upon occasions like this and talk to each other of the general conceptions which run all

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through the lives which they are living. There is no other reason for the existence of an association like this than that. And the essential idea of an association like this is the university idea, that men get together to compare diverse opinions, to learn of diverse conditions, to give each other the traveled mind. That is the business of the university.

Now there are certain specific things which the university can undertake by way of instruction, by way of definite information. For example, it can undertake the study of political and social conditions. And, as I was trying to expound at the outset of my remarks, political and social conditions are the gist of business; because, unless you understand the political situation, and more particularly the social situation, you cannot understand national taste and the processes of exchange, and you cannot be a successful merchant. You cannot know what kind of goods to send to what places unless you know a great deal about social conditions, and social conditions are often dominated by political conditions.

Now, mark you, I don't mean sociological conditions, because I don't know what sociology is (laughter); moreover I am convinced that there isn't a man living who does (laughter and applause); whenever a man is studying anything queer he calls it sociology (laughter). I guess that what is normal is good enough for me (laughter), and the best society not too good (laughter). But, for example, these people who call themselves alienists are at one of the centers of sociological inquiry (laughter). And the fellows who call themselves—I have forgotten what they call themselves, —the men who go around and live in penitentiaries and try to

get the point of view of the convict, and get it so perfectly that they can never get it out of their heads afterwards. I had one of these gentlemen—one of the most expert and curiously learned of them—spend a whole evening with me in my study, and when the evening was over I was hypnotized; I felt exactly as though it would be congenial to me to go out and commit a crime (laughter); I felt a certain hunger for irregularity, and I examined my state of mind with the curiosity of a surgeon using the scalpel. Now these fellows are sociologists, and I am afraid of them (laughter); they get me in an abnormal condition; and sociology, as I understand it, is everything left over after the political economist and the student of politics are through (laughter and applause). The political economist is talking about something definite and something in particular; the student of politics is talking about something definite and something in particular; but the sociologist is talking about anything he darn pleases. He doesn't define or limit his scope in the least, and he takes all the irregular things as his special, pet product. Now I don't believe that we have anything to do with that in a commercial community. We are after the regular thing. We have a zest for the regular thing, because a large market depends on the regular thing and not on the irregular thing. We are not selling to the abnormal person, we are selling to the normal person; and so I am not speaking of the study of sociology, but the study of society and the study of social conditions. By that I mean what men wear, what they eat, what their tastes are, what kind of patterns they prefer in their coats, what kind of goods they prefer next to their skins, the kind of fashions that they

have, the kind of prejudices that they have, the kind of old, wornout prepossessions that they have; all of that is pertinent to our imaginative conception of what the world is, and that is a subject matter of definite study, because men with their eyes wide open, men with the power of description and the power of explanation have gone up and down the world and found these things out for us. There is hardly a corner of the world into which at least a bicyclist won't go. There is hardly a part of the world into which some man won't go now out of sheer curiosity. He does not require any other or higher motive. He wants to go somewhere else, where nobody else has been, and the consequence is we are finding out a good deal about the dark corners of the globe. The light of information has gone into them, and there follows always the light of commerce. The moment the information comes, the commercial man is on tiptoe to know what he can sell to those people, and the moment that inquiry comes he is after his Consul and his Government to see to it that those doors that have been shut shall be open, and that commerce shall be allowed to get in there. He knows because he has read, and these fellows say, he has got something which these strange people want to buy. That is social study, and is susceptible of definite university treatment.

Then there is the study of political economy. I know that the study of political economy is not in as high repute as it once was, and that is the fault of the political economists, as Professor Laughlin will admit. The political economists allowed themselves a self-indulgence. The most self-indulgent thing you can do when you get hold of a complicated study is to simplify it and say to yourself,

"Now we are dealing with men in their economic relation, but it is too troublesome to bring in all the things they are likely to think about, and all the motives which are likely to move them: the taste of their wives; the preference they have for living in a particular region of the country; the predilection which they have for the trade of their fathers; all the sentimental things which come in; we cannot keep our books if we try to consider all those things; so we will simplify them and say that they will always follow the line of least resistance under the force of self-interest." That was the self-indulgence which the political economists allowed themselves. They said, we will reduce all this to a proposition of self-interest; all sorts of incalculable forces come into play which are not to be brought under the category of self-interest, and so political economy became an abstract science. So that one of its principal writers, John Stuart Mill, warned everybody that they were not for one moment to suppose that all of this was true in fact; that it was true in theory, but that it was not true in fact, and they were not to make the mistake of going out and passing acts of legislature according to the principles he put into his books. Men got into trouble by putting these abstract principles of political economy into statutes, because the limitation upon a statute is that it has to work; and if it is based upon too small a reckoning of human nature it is not going to work. We speak of ourselves as differing from the lower animals by reason of being rational creatures. But the rational process is the least thing about us. That is not, perhaps, a pleasant thing to think, but it is a fact. I take it that when we say that mind is king; we regard mind as one of the modern

constitutional monarchs who reigns but does not govern. There is a great tumultuous House of Commons made up of the passions which really runs the government, and the Prime Minister is the principal passion of the moment. It may be the money-getting passion; it may be the humanitarian passion; but it is a passion, and it is not rationalistic. It is not based upon the processes of logic, and government in all its institutions is intended for human nature, of which, as Burke profanely said, "The reason is the least part." And so we must understand human life in its complexity by social study along with the economical study, the two pieced together, in order to make a complete man for our consideration.

There is another thing that we can study. We can study that very interesting thing which is of comparatively recent creation. I mean economic geography,—the geography which divides the world up according to its products, according to what its soils produce, according to the place where its minerals are deposited, according to the various uses which it makes of its woods, and of all the things which are native and natural to it. Economic geography is, as it were, the map of the world for the student of commerce. It is also the map of the world for the student of human life in general, because, although I believe and profoundly believe in the regnancy of the human spirit, nevertheless everybody must admit that the economic rootages are among the most vital rootages of human life, and that according to a man's environments so is his life; that his economic condition governs his thought in nine cases out of ten. I had a long conversation the other day with a gentleman who was one of the most interesting

interviewers that I ever had visit me. He came to interview me, but I found that he liked to talk, and I interviewed him, and I think he went away with the impression that he had got my ideas, when he had only exploited his own. One of his ideas was this, that you can judge the reliability of a man by the latitude in which you find him. He maintained that lying was inevitable south of the 23rd parallel, and not knowing that I was from south of Mason and Dixon's line, he began to offer me his theory about the habits of mind of our southern people. He said, "Now you take a Southerner"—which interested me very much—"a Southerner has a self-indulgent habit of mind, which leads to his rhetorical use of language, because it is a great deal easier to state a thing vaguely and grandiloquently and with all the handsome flowers of speech than to state it precisely and come down to business. It costs a decided effort to make the mind a tool of precision, and your Southerner won't brace himself up to that effort, and so he fires at the mark, but he hits all around it as well as the mark itself. He fires with bird shot instead of with a rifle, and he brings down the game, but then he peppers the whole countryside in the process (laughter). That is a pure indication of indolence, and all you have to do is to increase that indolence a little bit, and you come to the belt of liars." (Laughter.) "For example, a Filipino cannot tell the truth, and it is for this reason. You ask him tonight, 'Will you do a certain job for me in the morning?' and, the line of least resistance is for him to say yes, because it saves him trouble. When the morning comes the easiest thing to do is not to do it, just to stay where he is. He is not

deliberately lying; it is the condition of the climate. (Laughter.) The climate has taken out of him the snap that there is in the truth." A very interesting theory of the morals of our fellow men which goes Buckle just one better. Buckle thought that men were made by the mountains that were around them and the plains that spread about them, and the heat and the cold of the climate in which they lived, and he thought that the Italian was no less moral than the Norwegian, though he had different habits in respect to various matters; that it was altogether a question of the influence of climate. Well, now, it is not merely a question of the climate, but the climate has a tremendous lot to do with it; and all the economic conditions which the climate and the products of the soil produce have a great deal to do with it; and all of these things are susceptible of definite study.

But it seems to me, gentlemen, that there is one thing with which the university can govern all of these things. After all, I suppose that in our sober, reflective moments we know that the object of life is not merely to set forward any particular piece of business, but that all pieces of business dovetail and fit and consort together, and that the real task that every man has set for himself is to give himself integrity of spirit and of purpose. The most pleasing thing to me about university life is that men are licked into something like the same shape in respect of the principles with which they go out into the world; the ideals of conduct, the ideals of truthful comradeship, the ideals of loyalty, the ideals of co-operation, the sense of *esprit de corps*, the feeling that they are men of a common country and put into it for a common service. After all, when you

have got that into a man you have got the root of the matter into him; you have got that one idealizing element which it is very difficult for the mere knockabout, hard experience of the world to give a man. If you put a man out into the world without first steadying him after that fashion, he is apt to think, is he not, that the hand of all the world is against him, and that the thing for him to do is to take every advantage against the world as the world seems to take every advantage against him? If he has never climbed some height of observation on which he can see the whole map of life, he is apt to stumble along its paths like a man without a direction and not to know what the end of his journey is. I believe that you will find that every great man of business has got somewhere, whether at the university or by his own birthright, a touch of the idealist in him, and that it is this touch of the idealist that makes the man, this conception of conduct as a whole, this love of integrity for its own sake, this idea of what he owes to the man in the other business, and to his rival or comrade in the same business, this feeling of the subtle linking of all men together, and behind it all the country itself, the country's welfare, the progress of America, and all the dear ideals which we are ready to leave our business for and give our lives to vindicate.

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